STUDIES IN QUALITATIVE METHODOLOGY VOLUME 6

CROSS-CULTURAL CASE STUDY

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PREFACE

Each volume in Studies in Qualitative Methodology publishes papers that follows similar themes relating to qualitative research. Earlier volumes have focussed on the conduct of qualitative research (Volume 1), research experience (Volume 2), the learning experience for a group of researchers who have conducted their first major study at Doctoral level (Volume 3), further first hand accounts together with commentaries on major issues concerned with qualitative research and qualitative researchers (Volume 4), and computing and qualitative research (Volume 5). This volume continues with some of the themes that have been raised in previous volumes. It focuses on personal accounts derived from researchers who have been engaged in conducting cross cultural, qualitative studies. The volume deals with many phases of the research process.

In common with other volumes in this Annual Review series, there is a brief introduction to contextualise the papers that follow. However, in common with many Annual Review publications and journals, editorial commentary has been confined to the introduction rather than through a lengthy essay review within the volume.

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Robert G Burgess
Series Editor

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INTRODUCTION

Qualitative research has come of age. There are now numerous texts, collections of readings and empirical studies that focus on qualitative research as well as book series and specialist journals that take as their focus qualitative research. In these circumstances, this research tradition has established itself in many countries, in particular in the United States and Britain. Ethnography and qualitative research now constitute major modes of data collection and data analysis in the conduct of fundamental research and evaluation. However, many of the essays and papers that are produced focus on research studying a single culture. In these circumstances, there are few essays and papers that deal with comparative study and cross cultural research.

This constitutes a problem as it artificially limits the boundaries of research studies. Second, it runs counter to research sponsorship as many opportunities now arise for researchers to engage in cross cultural studies. For example, research being promoted within the European Community focuses on studies involving several countries. In these circumstances, researchers need to consider the units of their study and the way in which conventional methodologies need to be adopted and adapted to the research problem in hand. As a consequence, many investigators who have been involved in researching their own society have now had to think about the implications of cross cultural study for different phases of the research process.

In concentrating on cross cultural qualitative research, this volume seeks to address a major gap in the literature. In particular, the essays cover a range of methodological and substantive issues. In common with many of the papers that have been published in earlier volumes of Studies in Qualitative Methodology they focus on the practice of social research. The papers bring together experienced researchers many of whom have worked together on joint projects. As a consequence, the papers reflect the issues that the qualitative researcher faces in defining a role. In addiion, there are issues concerning research design and the collection and analysis of data as well as its dissemination.

Each of the papers take up a major theme in the conduct of cross cultural qualitative research. The main themes include:

(a) The design of cross cultural qualitative research;

(b) Issues concerning the collection of data in cross cultural qualitative studies;

(c) The analysis of cross cultural research including the use of computers to analyse cross cultural qualitative data;

(d) Issues concerning the problems of cross cultural qualitative research and ways in which these problems have been overcome in actual projects.

All together, these papers focus on a set of themes that have been of central concern to experienced researchers conducting cross cultural qualitative research.

CROSS-CULTURAL CASE STUDY OVERVIEW

The chapters in this volume focus on different aspects of cross-cultural qualitative research. They are based on a range of studies that include topics in school-based education, higher education, trade unions and vocational training and education. They also provide examples of individuals working in their own countries as well as teams of researchers drawn from different countries working together. The chapters reflect a set of problems that researchers confront and different ways in which they have been resolved in the context of working on actual projects.

In the first chapter Jef Verhoeven provides a context for the volume when he poses the queston 'What is Cross Cultural Research'. His discussion and detailed examples drawn from three different interview based studies emphasize that cross cultural research is not just a question of coping with different languages, but is also about culture, meaning, dialect and geography. His chapter carries a warning that a common language is not a guarantee of success for cross-cultural research. Coffey and Atkinson also address issues of culture and understanding in the second chapter where they argue that ethnographic research is, by definition, concerned with cross-cultural under-standing. Given its roots in social and cultural anthropology and urban sociology, they see research, which is conducted within an ethnographic framework as standing at the crossroads of several intellectual traditions. They claim it is here that ethnographic research finds much of its creativity.

Issues of globalisation and the methodological challenges this poses for the researcher are raised in chapter three by Burgess. In addition, he focuses on definitions of 'the case' in cross-cultural case study by inviting us to consider its intellectual, temporal and methodological boundaries. The chapter stresses the need for agreement amongst researchers on units of study, principles of selection and organizing concepts. The chapter is important in helping us think

about the problems involved in focusing research in locations that at first glance may seem very different from each other.

Chapters four to nine concentrate on the actual experience of conducting cross-cultural case studies. Broadfoot discusses the challenges involved in her studies of curriculum and teacher professionalism in England and France whilst in chapter five, Kreslo, Rainbird and Vincent discuss the methods employed in studies of trades union policies towards vocational education and training, conducted across five countries. Both chapters discuss issues of language and meaning in the context of conducting interviews. They each offer interesting approaches to the problems of translation. Issues of language are also the concern of chapter six by Beuselinck, which focuses on studies of postgraduate education in different countries. Amongst the concerns raised is that of difficulties encountered in researching across different language communities within the same country. In this case Belgium. Chapter seven by Pole also disusses problems of interviewing and translation. Here, the context is a case study of curriculum development in Japan. In addition, the chapter examines questions of culture, etiquette and protocol in team based cross-cultural research. Chapter eight by Morrison and Benn draws on research conducted in schools in England and Denmark, which focussed on approaches to food and eating. The authors offer insight into different intellectual and disciplinary approaches to the topic, which in turn have implications for the design and conduct of case study research.

The final chapters of the volume focus on approaches to the analysis of case study material. In chapter nine, Wester and Peters ouline the procedures involved in using 'Kwalitan' software to analyze data collected in cross cultural qualitative research, whilst in chapter ten, Schratz considers issues of scope and culturally specific theoretical frameworks in the context of cross-cultural case study.

SOME REFLECTIONS ON CROSS-CULTURAL INTERVIEWING

Jef C. Verhoeven

Editors' Commentary: *Jef Verhoeven's chapter invites us to think about the definition of cross-cultural case study in the context of qualitative interviewing. For Verhoeven, the definition extends further than a concern with the technical aspects of language. Although he believes that a shared language is essential to cross-cultural interviewing, he argues for a definition which goes beyond geography to focus on culture in a wider conceptual sense, in which he locates issues of shared meaning and understanding. He warns, however, that cross-cultural interviewing and by implication, cross-cultural research more generally, is prone to an inevitable ethnocentrism where one of the parties involved believes they and their worldview is superior to the other. Verhoeven, a Belgian national, illustrates his discussion with reference to work conducted in the USA and Brazil.*

It is not my intention in this chapter to give a fully balanced analysis of the problems of interviewing in a cross-cultural context. My purpose is to reflect on some problems I met in different stages of cross-cultural interviewing in my own work and in one example provided by L. E. Thomas (1995). The first example will rely on my experience of collecting data in relation to the research practice and the meta-scientific opinions of third generation Symbolic Interactionists in the USA. (later called the American case). In this example I want to illustrate the problems of a researcher who is more or less familiar with the language and culture of the interviewee, but who was not really a member of that culture. The second example is taken from a short experience I had with one of my doctoral students in Brazil. I had only a passive

knowledge of Portuguese (could read and understand a little spoken Portuguese), but the interviews were conducted by the doctoral student, who spoke the language and was familiar with the Brazilian culture, although he was born in India (Goa) and received his education in India and Europe. I was only familiar with the culture on the basis of reading and conversation with local people. The third example, provided by Thomas, an American professor, is about interviewing an Indian Swami with a totally different cultural background. The Swami spoke English as the interviewer did, and at the same time the interviewer was helped by a native research assistant and an interpreter.

Without doubt, the cultures of the interviewer and the interviewees in the three cases are very different. Although these examples are clear as far as cultural differences are concerned, it should be stressed that cross-cultural interviewing might touch on other situations as well. It therefore makes sense to begin by asking: what do we mean by 'cross-cultural'?

Second, I will describe the three cases. Three problems will be discussed: (1) the preparation of the interview; (2) the interviewing process: mainly problems of creating good rapport, and problems of understanding during the interview; and (3) problems of interpretation.

Third, I shall discuss how these problems may be interpreted taking into account the analysis of the understanding of everyday life as conceived by Schutz & Luckmann (1974), and by Thomas (1995).

WHAT IS A CROSS-CULTURAL CONTEXT?

When I conducted, some years ago, a project on the process of choice of study of 12 to 14 year olds from working class and poor families, I found that at the beginning of the fieldwork, one of my collaborators was touched by the unexpected reactions and opinions of some of the families he interviewed. Their patterns of thinking, the values, and the behaviour patterns were sometimes very different from what he was used to as the son of a middle class family and as a university graduate. He spoke the same language as the interviewees, although these families were using a dialect he was not familiar with. This indicates that interviewing in a cross-cultural context is not only a problem of language, but also a problem of understanding a culture. Nevertheless, interviewing in a cross-cultural context may not simply be defined as a situation in which the culture of the interviewer and the interviewee are different. Not all cultural differences have the same influence on the interview situation, and even when the culture of both interviewer and interviewee seems to be very similar, it is often a challenge for the interviewer

to come to grips with the meaning of the language and the behaviour of the interviewee.

This problem becomes obvious when we look at the vast range of factors included in the definition of culture – a concept with a lot of meanings. Jim Thomas (1993: 12) refers to culture as: "... the totality of all learned social behaviour of a given group; it provides the 'systems of standards for perceiving, believing, evaluating, and acting' (Goodenough, 1981: 110) and the rules and the symbols of interpretation and discourse." But it also includes the material and symbolic artefacts of behaviour, such as belief systems, conceptual machinery for ordering social arrangements, and pre-existing structural and material attributes. In taking this definition into account, it may be expected that many interviewers are facing situations in which they are confronted with parts of cultures they are not really acquainted with. We may wonder whether an investigator trying to understand political behaviour, or the behaviour of workers on an assembly line, or the behaviour of prisoners, is also doing his work in a cross-cultural context. Indeed, it is very rare for a researcher to have had experience in parliament, or on an assembly line, or in prison, which would give a valid picture of the culture of these institutions. On this basis, a cross-cultural context should not be defined simply as a difference between the culture of the interviewee and the interviewer, but should be approached as a gradual difference between the culture of the two parties involved. Of course, it is clear that some cultural differences are easier to bridge than others, which determines the validity and plausibility of the data and consequently the analysis.

INTERVIEWING IN THREE DIFFERENT CROSS-CULTURAL CONTEXTS

Unstructured interviews may be characterised according to Burgess (1984: 102–103) as 'conversations with a purpose', in which 'the researcher is a friend and a confidant, who shows interest, understanding and sympathy in the life of the person with whom a conversation occurs'. Immediately he adds that 'this style of interviewing cannot be started without detailed knowledge and preparation', and he agrees with Zweig that "it is essential to observe people before a detailed conversation can occur." It is this kind of interview that I will examine in three very different contexts. First, I will describe the preparation of the interviews, second, the process of interviewing, and third, some of the problems concerned with analysis.

The Preparation of the Interviews

Methodologists do not agree on the way an unstructured interview should be prepared. Herbert Blumer (1969) contends that a researcher should approach the field of research with an open mind. Literature about the field of research might influence the way researchers look at the phenomena, and for that reason they should go into the field and try to examine it without categories provided by their reading. This is not accepted by many others, and I doubt whether it is possible to exclude knowledge-at-hand, and even whether it is wise to do so. McCracken (1988: 31), for instance, maintains that literature reviews give greater benefit than cost. After all, he argues, they give the researcher a critical insight into "the conscious and unconscious assumptions of scholarly enterprises," and help the researcher to construct a questionnaire.

Since the three projects I am discussing have a totally different field of study, the preparation of the interviews was also different.

The American Case
The purpose of this project was to study the links between European interpretative sociology and American Symbolic Interactionism. I was interested in the theory, the methodology, and the meta-scientific assumptions of the generation of Symbolic interactionists who got their training from Herbert Blumer. Moreover, I wanted to be informed about the social structure and the social networks in which this approach developed, the way these sociologists interpreted Symbolic Interactionist theory and methodology. In order to do this I started to read the work of these Symbolic Interactionists. I had the advantage having Tamotsu Shibutani, a former student of Herbert Blumer, as mentor when I was working at the University of California at Santa Barbara in 1980. Moreover, on the basis of previous projects I was also familiar with methodology, and with the philosophy of science, which would cover a substantial part of the interview. Relying on this literature I prepared an interview schedule for all respondents, which was adapted to the content of their work. What began as an advantage, later became a problem. Some of my questions were inspired by my philosophical and methodological reading, and seemed to confuse some of my respondents because they were not used to this language.

As far as the structure of American society and universities was concerned the only introduction I had, was some short visits to the USA, my reading, talks with American friends, and a four month stay in Santa Barbara before I started interviewing. But again, I could rely on a lot of information from Tom Shibutani in relation to university life, and on his acquaintance with the

Chicago scene at the end of the 1940s. He also had a good relationship with most of these scholars, which made access easy.

A knowledge of the English language was also important for this project. English is not my mother-tongue, but because I was used to reading English books and had some experience of speaking English, I was prepared to do my own interviewing, although I have to admit that my vocabulary was sometimes insufficient to understand immediately what the interviewee wanted to express.

The Brazilian Case

In 1987 I had the privilege to teach for a month at the University of Curitiba (Parana) in Brazil, and I took advantage of that opportunity to do some field work with one of my former doctoral students. During several years, he had worked under my direction in Leuven and for many reasons he had not finished his dissertation. His dissertation was about curriculum development in Brazilian secondary schools. In the meantime I was working on a project about school management in Belgium. As he had some difficulties in doing his fieldwork, we thought it might be interesting to do some work together and write a comparative piece on Belgian and Brazilian school management. My visit could bring us together in the field, give me a better understanding of the problems he was confronting, and provide me with a better understanding of the Brazilian educational system. During the time he was in our department, he told me about the Brazilian educational system and I was familiar with his research design. This was not sufficient to do field work in Brazil. In order to do it properly I had to study Portuguese. At the time I arrived in Brazil I had a passive knowledge of the language, but not enough to understand a conversation without problems. Consequently, I had to rely on my doctoral student to do the interviewing. We prepared the interview schedule together in English, but it was his task to conduct the interviews in Portuguese. I would accompany him and intervene in the interviews only when it was necessary. One of his students contacted the headmasters of four schools in Curitiba, and all of them were willing to meet us. The aim of this initial contact was to collect some general ideas about the school and school-based management.

The Indian Case

L. E. Thomas (1995) was interested in the question "why some people, as they grow older, become fearful and bitter, while others, who suffer the same physical insults and personal losses, age gracefully?" In another project he found that the elderly in England and in India, who are sensitive about a world view in which there was room for a transcendent dimension were more likely

to age gracefully. As he wanted to collect more data in relation to this problem he went back to India to interview and observe a dozen religious renunciates in Varanasi, the sacred city of Hinduism. L. E. Thomas is an American professor of human development and family relations. He was acquainted with the Vedic world view, studied yoga, and practiced Hindu meditation techniques. But even with this preparation he admits that he found the beliefs of Swami Tananda, an interviewee, strange and personally repugnant.

Who is Swami Tananda? He is an Indian aged seventy years, who spoke fluent English, was trained in Western science, and taught in a government college before his retirement as a Swami in a Shiva temple. In spite of his knowledge of Western science and thinking, he was strongly opposed to Western ideas.

In order to do the interviewing Thomas was assisted by a local research assistant and by an interpreter. They gave him a lot of interesting data, although he did not need them as interpreters to interview Swami Tananda.

The Interviewing

As with other unstructured interviews, cross-cultural interviews should take place in a sphere of openness and understanding. Already, from the start an interviewer should show that he is interested in all kinds of information the interviewee can provide, and that there is no reason whatsoever that the interviewee will loose face when some answers do not fit the ideas of the interviewer. Important here is not only the words spoken by the interviewee, but body language as well (McCracken, 1988: 38). The interviewer needs to be aware of the influence of attitudes towards the interviewee.

The three projects developed in a very different way as will be shown.

Interviewing in the USA
My contact with the interviewees in this project was very friendly. Most of the people I wanted to visit, were willing to co-operate, and most of them offered a day or more to do the interviewing. Some of them even invited me to stay in their home. I will illustrate my work by describing the circumstances of three interviews.

Jerry[1] invited me to stay with him in Smalltown. I arrived in the afternoon before the interview, and Jerry came and got me at the airport. It was the first time I had ever met him, but he was very nice and cordial. The night before the interview we spent together with him and his wife, and had dinner in a local restaurant. At the end of dinner, he took me to the kitchen of the restaurant and

asked me to watch the people in the kitchen. For him this kind of observation was 'real sociology'.

It was my intention to start my interview at 9 a.m. the next day, but he gave me some other papers of his to read, and it was almost noon before we began. We had no break for lunch. Between us we had a carton of beer and that was all we had until 5 p.m. when Eleanor, his wife, came home from work. The interview was only interrupted by phones, and problems with Jerry's tape-recorder. Like me, he wanted to tape the interview.

The interview with Bob was different. I conducted the interview at his Summer residence in Sunnytown, and apart from interviewing him we had lunch together with his wife. I did not have the same opportunity to observe Bob or to chat about other things as I did with Jerry. Because Bob was one of the latest on my list, he was already informed by his friends about the content of my interview. My introduction was very short. The interview went very smoothly, and I had the feeling that all the themes I intended to discuss, could be covered.

Chris invited me to come to his office at Megatown. The place of the interview was in comparison with the others formal, and the interviewee had planned other tasks the same day apart from seeing me. It was a friendly meeting but in comparison with the others much shorter, probably because Chris was not so fond of Symbolic Interactionism. There was not very much time for an informal introduction. I could ask the questions I wanted, but I knew that the time was limited. Consequently, my questions became shorter, and the data I collected was less than in the other interviews.

Although the interviews went smoothly the cultural differences between interviewer and interviewee had consequences for the interview as will be illustrated in the next section. As far as rapport between interviewer and interviewee was concerned, I had the feeling that this did not influence cultural differences for the worst; probably, the interviewer being a foreigner made it easier to speak about 'hot topics'.

Interviewing in Brazil
Four schools were contacted by the research assistant, and in three of the schools we had the interviews with the headmaster as was promised. In one of the schools the headmaster had no time to see us, but delegated the interview to two deputy heads and a secretary. In all schools the interviewees were very kind and gave us the opportunity to visit different parts of the school. They asked us why a Belgian came to visit the school, and were very honoured by the visit. As I have indicated, my incapacity to speak fluently Portuguese, and the fact that I could not easily understand the local accent, was a big handicap

during the interview. I was supposed to intervene when the questioning was weak. Sometimes I did, but it was certainly not enough according to the interview transcripts. Actually, it was not a big problem for the project because these interviews were only a first step to more fieldwork.

In conclusion, we can say that the quality of the interviewing was influenced by the lack of a sufficient knowledge of the local language by one of the interviewers. Moreover the short time I had to familiarise myself with the field hindered a good understanding of the field, and made it impossible for me to be critical enough towards the data collected: e.g. one of the headmasters offered dubious information, and it was only after the interview that I was aware of it. This experience convinced me that cross-cultural interviewing is only possible when interviewee and interviewer share a common knowledge (for similar problems see Cicourel, 1974, and for problems of translation see Werner, Schoepfle, 1987: 354–379).

The Indian Case
L. E. Thomas (1995) does not give very much information about the course of his interviews. We only know that these interviews took several months and that the interviews with Swami Tananda occurred in a friendly way, even when the Swami was denying mutual respect to the interviewer as a human being. On the other hand Thomas contends that even the denial of his humanity by Tananda was no reason for him to stop interviewing, and he believes that he was able to understand Tananda correctly.

Problems of Interpretation

The three examples show that cross-cultural interviewing is possible, but only when certain conditions are met. The Brazilian case, although the information was interesting, shows how language is a very important means to come to a good understanding of the situation. It also became clear that an investigator needs a reasonable period of time in order to become familiar with the field. Since this was not present in the Brazilian case, the following analysis will be confined to the American and the Indian case.

The American Case
In this case it is important to remember that I started this project after I did some work from a methodological standpoint and from the standpoint of the philosophy of science. This had such an influence on my questioning that I tried to push my interviewees to take a stance in relation to points of discussion which were considered to be important in the philosophical literature. Doing

this I had forgotten that most interviewees were not much interested in fundamental methodological problems, but were more eager to do empirical work.

The first quotation shows that Jerry was stimulated to reflect on the concepts realism and idealism. His answer shows that he was not acquainted with these concepts in a technical sense (Jerry calls them Greek concepts) (see Jerry (a)). He stresses his empirical stance, but does not follow the interviewer when he wants the interviewee to choose between falsification and verification, after he had declared in one of his books to prefer falsification, whereas in most of his books he seems to be more interested in verification (see Jerry (b)). He also refuses to think about himself as having assumptions which might influence his work. In the same quotation it is obvious that a person who is not accustomed to working with a foreign language (German) gets confused, even when this language is used in relation to ideas he himself has formulated. After all, Jerry translated a German book into English.

A similar reaction is demonstrated by Chris. He is aware of the presuppositions, but admits that he does not pay attention to them. And when he is pushed to put himself in the tradition of American Pragmatism he is very outspoken about his limited knowledge of Pragmatism. He also refuses to discuss his position towards realism and idealism, and suggests that nobody cares except when you care about "words," not about "action."

Not much difference may be found in the response of Bob on the question of what he considers to be truth. Bob also refuses to follow the interviewer in his philosophical approach and starts to speak about "credibility, believability."

In relation to these statements we can ask whether this refusal by the interviewees has to do with the American background or with their typical views about doing research. Whatever the answer may be, it is obvious that interviewer and interviewee come to a good understanding of each others opinion.

Jerry (a)

"I: When you are speaking about society, or about persons, do you see them as a reality? Or just as an imagination of the researcher?
J: Well I think imaginations are real. So I have no problem.
I: Oh. Uhhmm. (shared laughter). It destroys my classical, perhaps Greek concepts. There is always this difference between idealism and realism. (laughter).

J: Oh yes. Well, I mean I really got into this in the study of clothing, you
      know. Because people talk about mere appearances. You know, or it's
      merely an appearance. These appearances are quite real. (laughter).
I: Now let's go on and . . .
J: You see at bottom, d . . . , I'm an empiricist.
I: Yes, I see. In this sense you are realistic. Now I have here some special
      questions in relation to some of your articles."

I quoted a German paper published by Jerry in which he seems to say that he
does not want to join the authors' assumptions he is discussing. Asked by Jerry
to get the context, I read the German text aloud, which did not make it easier.
After offering an English version, I suggested that Jerry probably wants to
stress the empirical approach instead of accepting the assumptions made by the
authors. Jerry agreed:

"J: That's what I am saying. You know the reason that I am upset and
      destroying all (laughter), all this equipment (tape recorder) is because I've
      never been pressed this far, you know. And I'm wondering whether I have
      some basic assumptions. I like to think I don't.
I: You like to think you haven't?
J: Yes.
I: But do you think that's a realistic statement?
J: No. Of course not. You know what I like is not necessarily reality."

In the next quotation I try to find out whether Jerry was influenced by Karl
Popper's (1959) idea of falsification, because in one of his books Jerry speaks
about the graveyard of theories. On the other hand Jerry does not speak about
falsification but verification.

Jerry (b)

"I: Must I say then, that development of science is more, or your view of
      developing science is more to formulate hypotheses, or to formulate new
      questions, rather than saying this is now the end of . . .
J: Oh yes. Definitely. The problem is, why I even state in the beginning of
      that book which I wrote, you know, that science is the graveyard of
      theories, but the problems continue. (laughter) Which is fascinating.
I: Now. May I bring in the philosophy of science, speaking about . . .
J: No. I'd rather you didn't. (laughter)
I: Was there any moment that you said I have to look at the work of Popper,
      or is there any relationship?

J: No. What happens to me in that respect, you know. Several people told me to look at Popper, you know. And I know at the same time that Popper is very fashionable. Now I tend to despise fashion . . ."

Jerry continued to ridicule all kind of fashion in philosophy of science, and refused to follow my suggestions. In his opinion verification is the proper way to make progress in science.

I took for granted that all researchers would be aware of presuppositions they have in relation to man and society which could influence the methodology of a researcher. I also thought that all sociologists related to Symbolic Interactionism were well acquainted with Pragmatism. Chris denied both hypotheses.

Chris

"I: Now speaking about these presuppositions. What kind of presuppositions do you use in your work in relation to the person, and in relation to society.
C: I don't use any consciously.
I: (laughter) Not at all? You have no presuppositions?
C: No conscious presuppositions. No. No formal ones.
I: Except the fact that you mentioned some presuppositions in structural and common sense individualist stances (in one of his books). There you see clearly some presuppositions but according to your answer you don't have them at all, clearly.
C: No. I'm not formally preoccupied with them. I'm sure they are there. Whether they are consistent or not is another question and I wouldn't be surprised if they were inconsistent because it depends on the problem, and what I happen to be thinking about at the time, or what I happen to read at the time.
I: Is this stance part of your pragmatical stance? American pragmatism?
C: Yes but again it's not formal pragmatism. I've never read anything about pragmatism. Well I guess I read Mead in the 'History of movements in 19th century thought'. That was years ago."

Further, he stresses that as a researcher he is not interested in philosophical questions, but admits that he refuses to accept a pure idealistic or realistic standpoint.

"C: By all means. If I . . . well part of my feelings that formalizing these things, in a sense, polarizes them also. I cannot accept a philosophical realist position any more than I can accept a philosophical idealist position. If I

were absolutely forced to choose I suppose that I would have to support realism, certainly my tendency is in that direction. But I resent the necessity of choosing and I think they are created by words. There may be some critical points I suppose in which a choice has to be made. By and large I think the necessity of choice is created by people who are occupied with words, not with action. (. . .) Anyone would be an imbecile adopting a purely idealistic, or a purely realistic situation. I mean everything we see about us – you know both are involved in the world. But as I say certainly if I have to choose, and I would resent the necessity, it would certainly be toward the direction of realism. Hume is far more compatible to me than Descartes. Without a doubt."

The question of truth is a central problem in methodology. How to attain truth is a problem each researcher is facing. Therefore I started to question Bob from a very traditional standpoint about truth, which he refused to accept.

Bob

"I: Now, do you mean that within these discussions about truth: it's hard for a sociologist to get truth. Is truth something . . .

B: See. I didn't say truth (laughter). That's the point: I didn't say truth. I don't know what I think about truth. I know that's a tangled, philosophical business before I could say anything about it I would have to read about fifty books that I haven't read. I don't say truth. Maybe some day I will read all these books and get into it. I do say truth in another connection about photography. Did I send that to you.

I: That's right, I have a copy of that.

B: But it's essentially the same kind of analysis. In talking about it, it's not so much truth as credibility, believability, persuasiveness. I think of science as a branch of rhetoric in the old fashioned sense as the art of persuasion.

I: That's one of the things you are very strongly attached to. That's one of the things you are very involved in that idea of credibility in a different . . .

B: Yes. I think it's a more useful way of talking about it than truth because I think of what I just said before, in your last question. What we are engaged in is persuading other people that we have found out something that they can believe, that they can depend on. Something credible. So that's why . . . credibility, believability."

My starting point of approaching all publications as if they were realised on the basis of a very deep philosophical reflection could not be accepted by all

interviewees. Bob, for example, opposes without hesitation when I try to find a particular model of scientific research in a sentence in one of his books.

Bob

"B: Wait a minute. The best evidence may be that gathered in the most unthinking fashion (Bob is here referring to a sentence in one of his books). Because there might be less bias produced by the wish to substantiate or repudiate a particular idea.

I: The accent is on may be . . . ?

B: Yes. It could be.

I: It could be.

B: That the more you plan it the more danger there is of finding what you set out to find. And the less you plan it the more likely you will find something which is actually something that could be used to disprove some favourite idea you have.

I: Yes I see. So it's more relativistic than I put it here.

B: You see. You see what you are doing is treating these things that I say somewhat more seriously than they deserve to be treated. That's to say they are not really philosophical dictums. What they are, how shall I put it, bits of practical advice. (. . .)

All right now but look you really misread that sentence. Because the whole sentence is that it might be that because and the because is, is that this way you wouldn't just find what you set out to find. You see. And that's quite different from saying, it's best to do it. I mean, the way you quoted it makes it sound as though I am recommending that you should never think. I think it's very important not to do it too early. (. . .) My version of that (evolutionary epistemology) is we shouldn't get rid of an idea too quickly. There will be plenty of time to say after all it's no good. But if you get the idea, at the same moment you get the idea you dismiss it. Oh well that's not right – everybody knows that's not right. That's a mistake – so it's better to entertain many contradictory ideas, entertain many ideas, eventually you can work them out and decide which ones to get rid of.

It was not only the interviewer who took his own standpoint too strongly as a starting point in the interview. The interviewees also took for granted the classical American concepts or their own standpoint. This was the case in the next response from Bob who explained to the interviewee the difference between liberals and radicals in America, concepts which have a different meaning in the European tradition.

Bob

"B: (. . .) So, uhh . . . yes in that sense anything that shakes things up is in that
     sense, radical. I don't know if I would use that language, you know,
     radical. It's very vague. That interview was a creature of that time. That
     was a big issue for everyone – you can't imagine what happened in
     American sociology in the middle and late sixties when people were
     running around accusing each other of being fascists and capitalist lackeys
     and all this stuff. For a lot of people to be, let's say, Hubert Humphrey-
     Democrat, they considered that equivalent to being fascist.
I: Was that the attitude? It was a time that . . .
B: Yes. Liberal was a bad word, it's still a bad word. I remember a friend of
     mine, J. S. who teaches Criminology, hit on the perfect way to deal with
     this charge. He would say: "Yes I'm a Liberal – now what?" It was very
     confusing because nobody would believe that anybody would accept such
     a description of themselves.
I: Liberal is the equal of Radical?
B: No. I mean, the idea is, you know, I mean it's roughly the equivalent of
     reform and revolution. The liberal is someone who wants to change things
     for the better but go slow."

Jerry on the other hand refuses to accept the concept of behavioural science (by
the interviewer used as a general concept for all sciences studying in human
behaviour), because he denies the validity of the research conducted by some
representatives of 'behavioural science'.

Jerry

"J: There's no behavioural science.
I: There's no behavioural science.
J: No. Absolutely not.
I: But when I say behavioural sciences . . .
J: No sense at all. No sense at all.
I: Why not? Explain that.
J: Because (laughter) What is behaviour? Do I . . . I mean now you study
     behaviour as a change in motion? of the object? Physics talks about that.
     Talk about behaviour as some change in the organism. Biology can handle
     that. Must we bring physics and biology together? Do you want to do
     that?
I: Uu mm.
J: Well. We have to be concerned with meaningful conduct."

The Indian case

L. E. Thomas approached a world which was totally different from his own. He was helped by his research assistant, Omji, to gain a better understanding of the peculiarities of the Veda culture, the life-world of Swami Tananda. Two important challenges he had to face: (1) the concept of truth used by the Swami; and (2) the Swamis concept of human. Both were a total denial of Western thinking and values, which had to be bridged by Thomas in order to grasp the thinking and behaviour of the Swami.

The discussion of truth started by an explanation of the inscription on a Rupee note 'truth alone shall prevail'.

"T (Swami Tananda): There are so many opponents who at first you think are going to win. What are its opponents? It is your rational thinking. The different religions. They say they are going to win. Our truth is the eternal knowledge, the Vedas. It is not known through rational thinking. It is absolute authority, which you get through tradition. That eternal knowledge supersedes all others.

Suppose there is a rope and somebody understands it to be a serpent. The existence of the serpent is something imaginary. The rope is existence. The serpent exists because of the rope. The image of the serpent is dependent for its existence on the rope. So if you understand the rope, the serpent disappears. Only one thing remains. And that is the truth." (. . .)

"I: (Interviewer): It seems to me what you have done is to say that there are two classes of people: those who see the truth, and those who see falsehood. So you have made a dichotomy.

T: (Sw. Tananda): Those who are seeing the rope will never be defeated by somebody who sees it as a serpent. Someday it will come that those who are thinking that it is a serpent will have to realise that what that person is saying is correct. So there is only truth. There may be some who think it is a serpent, and another group who think it is a garland. Between these two there will be a fight. But the one who is thinking it is a rope, no matter how the fight goes, will eventually win. That is reality. There are so many ways he can show that it is nothing but a rope. There is no question of anyone disproving him.

I: So he has the truth. And he is the only one who has the truth?

T: Yes. He is the only one who can have the truth. The truth in this particular case is the traditional learning that comes from the Vedas. We put the intellect under the Vedas, under the eternal knowledge. So our knowledge is always depending on the Vedas. Western man is just exploring. We don't explore; we go to the target directly.

I don't read books; they are irrelevant. If they agree with the tradition, then they tell you nothing new. If they don't agree with the tradition, then I reject them. If you are trained sufficiently, you don't have to read anything more. You can't know if something is valid except by the tradition. Otherwise you are placing yourself in the position of making a decision based on your intellect. It is blasphemy to put one's intellect above the Vedas." (Thomas, 1995: 101–102).

Thomas stated that Tanandas world view is totally different from his; it is almost incommensurable. The Swami relies on the Vedic epistemology that higher knowledge is "not reached by systematically working up through lower knowledge, but apprehend all at once, intuitively." Nothing can change this truth: perception nor reasoning. Empirical verification does not change anything of the truth, an idea totally opposed to our Western thinking. Books have no value whatsoever for a good understanding of the world; only tradition (Vedas) is worthwhile for life. This is the irreconcilable cleavage between the life-world of the interviewer and the interviewee. 'East is East, and West is West', nothing will change that.

Although these ideas are not easily tolerated by a Westerner, Thomas was more insulted by Tanandas ethnocentric ideas about the humanity of Western people. In the next transcript the Swami denies that the interviewer is human.

"T. What do you mean by 'human'? Only a Brahman is fully human. He benefits from the karma that he is now making in his life. Only the Brahman follows Manu. You don't follow Manu.

I: You don't really mean that I'm not a human being. You respect me as a human being, and you know I don't follow Manu.

T: Why should I call you a human being? As I told you, the human being is supposed to sow his karma, and reap the fruit of it. And the capacity to sow is vested only in those who follow Manu. And others can only enjoy it.

I: Is that really true?

T: It is very much true. That is why we say that India has a particular property. The Vedas are meant for Indians. It is by them that you can sow your karma, and reap the fruit." (Thomas, 1995: 104).

Thomas admits that though he was familiar with the Vedic thinking, he had to realize that this extreme orthodox Veda standpoint was not part of his knowledge about this culture. The most difficult moment was when he became aware of the total denial of his value as a human being by Tananda. He could not understand why a person who did not see him as a human being could communicate with him. Moreover, he was horrified by the social and political consequences of this philosophy. According to the Vedic thinking democracy is

impossible and all Indian policy makers opposed to the Vedic world view should be thwarted. In spite of all these humiliations Thomas continued his research and maintained he had a grasp of this interviewee. How is this possible?

HOW TO UNDERSTAND EACH OTHER IN CROSS-CULTURAL INTERVIEWING?

Understanding the interviewee in a cross-cultural context is according to L. E. Thomas (1995: 106–110) not only a question of the "the ability to construe their symbol systems" as C. Geertz (1979) has argued, but also a question of mutual respect. Thomas thought at a certain moment (and he gives several indicators) that his relationship with the Swami was an I-Thou relationship, but the utterances of the Swami at the end of his stay confirm that the Swami saw this as an I-it relationship (M. Buber, 1983). This created a strong tension between both. Cross-cultural interviewing is always vulnerable because of the ethnocentrism of one of the parties; one party believes to belong to a better group than the other and this might hurt the relationship between both.

Although this problem might happen more in a cross-cultural situation when the interviewer and the interviewee belong to cultures opposed to each other, it might also occur in other interview situations. Some years ago when I was interviewing Erving Goffman about his work and the structural background in which it came to be developed, he told me that it made no sense for a sociologist to spend his time doing the work I was involved in (Verhoeven, 1993). This statement probably did not hurt me as much as the statement of the Swami harmed Thomas, but it was to a certain extent a negation of what I was doing. Although this statement did not contribute to a better rapport between interviewer and interviewee, it was no reason for me to stop the interview. I saw it as an indicator of Goffman's opinion on the main task of sociology, or better, that doing a sociology of sociology was not one of his main interests. To me the emotional link with the interviewee was of less importance than it was to Thomas. Depending on the field of study and the psychology of interviewer and interviewee mutual respect of interviewer and interviewee might have less or more relevance and might influence the rapport.

Thomas (1995: 111) explains his capacity to understand the interviewee by relying on a distinction made by H. G. Gadamer (1975). The latter differentiates between "the high level abstraction of world views (or paradigms) and knowledge related to practical discourse and action (praxis)." In his opinion paradigms may be very unlike and make communication difficult. Nevertheless, this makes communication not impossible, because

these paradigms are not the only part of life. Absolute knowledge is not available. We are "finite historical beings who are always 'on the way', in a world of constant flux and ambiguity." This makes the understanding of different paradigms difficult but not impossible. We are confronted with different horizons, and the knowledge of these makes it possible to understand the other paradigms. We have an experience of a common humanity in our daily life and this enables us "to move beyond our 'blind prejudices'." This facilitates communication even when we do not share the same paradigm. Contact with other horizons or paradigms capacitates us to come to a better understanding of the other. Only through the others we attain a full understanding of ourselves. According to L. E. Thomas, this is what makes mutual understanding of interviewer and interviewee in a cross-cultural context possible.

Although Schutz & Luckmann (1974: 289) are more interested in the question of how members of the same society come to a mutual understanding, they give an interesting example in which we may see a prefiguration of the way an interviewer and an interviewee in a cross-cultural context come to grasp each others viewpoint.

"We had assumed that a man and a woman from different races and without a common language were stranded on a desert island. Each brings with him a different stock of socialized interpretational and motivational relevances. But a 'common' fate is imposed on them. A wide range of typical problems confronts them both. Whoever 'independently' finds the 'solution' to a definite everyday problem (or has already 'brought' it with him) can transmit it to the other person. This will probably first involve 'objectivations' on the presymbolic level. In the course of continuing we-relations, due to compelling motives and with the help of the two languages 'brought with them', a common language will develop in which they can share relatively 'ideal' and 'anonymous' knowledge with each other. Thus, we now have a 'society' consisting of two people: a 'common fate', i.e. typically similar problems, a factual social structure, in this case limited to the we-relation, a common language, and a common stock of knowledge." (Schutz & Luckmann, 1974: 289)

They continue by stressing that although both share a common stock of knowledge, each has a stock of subjective knowledge which is not 'objectivated'. Each meets problems which are typical for the woman as a woman, and for the man as a man. Nevertheless, both develop a common quasi-stock of knowledge which make it possible to understand the stock of subjective knowledge.

In this example it becomes obvious that two people with a different cultural background develop a common language in a we-relationship. It is this we-

relationship which is one of the first experiences of children in a family that confronts them with typifications, which in principle are conceivable without language ('prelinguistic experiences'), but later on are transmitted by language. "The language is a system of typifying schemata of experience, which rests on idealizations and anonymizations of immediate subjective experience" (Schutz & Luckmann, 1974: 233). Language is such an important device to understand the life-world. It is so important that "the largest province of lifeworldly typifications is linguistically objectivated" and changes in the social relevance of given experiential schemata are followed by changes in the meaning of a language. This linguistic experience is part of the earlier we-experience of children which is immediately linked with facial expression and gestures of the Other. Hence language appears for the child as a direct experience of his fellows. When this language has become part of the personality of the child, "it becomes to a great extent independent of the concrete we-relationship and of the immediacy of experience. Language can then provide knowledge about realities which not only transcend the current experience of the individual, but also are practically, if not also in principle, inaccessible to him" (Schutz & Luckmann, 1974: 249). But since the we-experiences are different among children of different classes, nations, ethnicities, and so on, the language and the understanding of the life-world will be different for them. Only in societies where the typical contexts of experience and act would be the same, where roles are not differentiated, only in these societies the social stock of knowledge would be the same. Consequently, understanding the other belonging to an other culture will be not easy, yet not impossible.

This is almost like the situation of cross-cultural interviewing. An interviewer and an interviewee who have grown up with a different stock of knowledge have the challenge of bridging this gap. They do not have the time to pass through the same stages a child is going through. They have to try to build a we-relationship which is hindered by their different stocks of knowledge. On the other hand they can rely on a common language which makes it possible to grasp the typifications available in each culture. When this is not available, as in the Brazilian case, cross-cultural interviewing does not or only partly attains the purpose. But even when both interviewer and interviewee share the same language, as in the American and the Indian case, mutual understanding is not without problems. A common language, although it is linked with a particular life-world, is no guarantee to grasp everything easily. When the interviewer and the interviewee do not share the same pre-linguistic experience a more or less wide gap has to be bridged. To reach this aim is the continuous challenge of cross-cultural interviewing.

NOTE

1. Pseudonyms of the interviewees and of the places are used instead of the real names.

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HECATE'S DOMAIN: ETHNOGRAPHY AT THE CULTURAL CROSSROADS[1]

Amanda Coffey and Paul Atkinson

Editors' Commentary: *For Coffey and Atkinson cross-cultural case study is synonymous with ethnographic research. They locate ethnography as standing at the crossroads of social and cultural anthropology and urban sociology. In this sense, research based on ethnography draws from these different intellectual cultures and hence, the social understanding, which it facilitates, is by definition cross-cultural. Moreover, they argue that much of the creativity, which characterizes ethnographic research, is found in the tensions and juxtapositions between different intellectual cultures in the social sciences and humanities. The chapter is concerned with cross-cultural methodology rather than method, within which Coffey and Atkinson discuss processes of data analysis. In particular, grounded theorizing and the integration of qualitative software packages are pointed to as having a universalizing impact on data analysis.*

CULTURAL DIFFERENCE AND CULTURAL INQUIRY

We have two contentions in this chapter. First, ethnographic research in sociology and anthropology – classically associated with cross-cultural research and cultural translation – is itself a cross-cultural intellectual enterprise. Secondly, recent accounts imply that the cultural diversity of ethnography is a recent phenomenon: we suggest by contrast that it has long been characterized by competing cultural tendencies. We suggest that ethnography itself is recurrently poised between different academic cultures. It does not merely span different academic disciplines (anthropology, sociology

and various applied areas). Ethnography also draws on competing intellectual inspirations – humanistic and scientific. The cultural diversification of ethnography has been accentuated in recent debates and accounts, but the underlying tensions are not dependent solely on contemporary trends (for example postmodernism, poststructuralism, postcolonialism, (post)feminism). Such trends reflect more persistent sources of ethnographic diversity. The cultural paradox of ethnography is the continuity of tensions and diversity. In this chapter we use this contention to illuminate the ways in which ethnography has been and remains poised at a cultural crossroads.[2]

For the purposes of this chapter we take it that cross-cultural inquiry means something fairly self-evident to most practitioners and readers of social-science research. Disciplines such as social or cultural anthropology or development studies seem perhaps to promote cross-cultural research *per excellence.* There are many studies in sociology, education, health research, political science and cultural studies that would also fit into that taken-for-granted view of cross-cultural research. It is a commonplace to note that ethnographic research has been of particular significance in cross-cultural research, and has been widely used to that end. Grounded in the traditions of social and cultural anthropology and urban sociology, ethnography has frequently been justified in terms of 'cross cultural' understanding, or principles of cultural translation. In the course of this chapter we adopt a slightly different stance toward ethnography and the cross-cultural. Rather than focus on the juxtaposition of the cultural differences between the ethnographer and 'the other', or on problems of understanding 'other cultures', we emphasize the extent to which contemporary ethnography is itself an intellectual enterprise that stands at the crossroads of several intellectual traditions. Ethnographic research, then, is an inherently cross-cultural or interstitial academic activity. Contemporary ethnographic research finds much of its creativity in the tensions and juxtapositions between different intellectual currents in the social sciences and in the humanities.

We take it as axiomatic that 'cultures' do not exist *a priori,* waiting to be discovered and compared independently of sociological or anthropological investigation. It is not necessary to endorse an extreme version of constructivism or relativism in order to recognize that 'cultures' or 'sub-cultures' are constructs of social-scientific analysis. Whether cultures are conceived of as texts, enacted performances. stocks of knowledge, actors' practical methods, or as constellations of rules, norms or values, these are all analysts' methods for constructing cultures as objects of academic discourse. Cross-cultural research in itself, therefore, does not deal with stable and pregiven entities. Equally, the conceptual frameworks used to do that analytic work are not themselves stable or homogeneous. The methodological and aesthetic devices that are used to

understand cultural processes and cultural differences are, reflexively, part of the domain of culture. Academic cultures are no more 'given' than in any others, whether they be academic disciplines or particular theoretical and methodological traditions.

Our task here is not simply a matter of contrasting an established ethnographic orthodoxy with various 'alternatives' – although that might be one way of understanding contemporary methodological stances. Rather we suggest that 'orthodoxy' in ethnographic research is not a stable category. On the contrary. the lasting vigour of ethnographic research owes much to its diversity of methodological and representational standpoints. The postmodern turn in ethnography, and in the social sciences more generally, has inspired many commentators to identify and to explore a varied range of ways to report and represent the social or the cultural. Later in this chapter we explore some aspects of this diversity and their consequences. We do not, however, believe that it is necessary to invoke the rhetoric of postmodern inquiry in order to take these issues seriously. Although the postmodernist turn has often provided the inspiration, earlier – more classical – versions of sociological or anthropo-logical understanding furnish justifications for the exploration of ethnographic representation. In other words, there is no need to appeal to 'postmodernism' *per se* to account for the diversity that characterizes the ethnographic enterprise. We believe that the tensions we outline in the course of this chapter are integral to ethnography. The processes of cultural translation do not traverse only the cultural boundaries of the ethnographer and her or his subject-matter. Ethnography is also shot through with cultural differences. The tensions and differences that we explore are inherent to the development and promotion of ethnographic research.

Current perspectives on ethnographic and cultural research can be charac-terized in terms of variety. The methodological domain is marked by a clamour of styles and justifications. Not only is there diversity, there are also subversive and transgressive tendencies. We can think in terms of contrast and complementarity in research methods and strategies: we must also think in terms of contested approaches to social and cultural research. Contemporary ethnography certainly cannot be seen as an unproblematic set of procedures for data collection. Indeed, it cannot be seen as a purely methodological category. The ethnographic enterprise now carries with it connotations of theoretical, epistemological and ethical controversy. Contemporary diversity is not, however, a recent offshoot from a previously uniform research culture. It is not the case that a homogeneous 'modern' ethnography has had to await the rise of fashionable postmodernity. Postmodernity has undoubtedly amplified certain differences, but has not supplanted a unified stream of research methodology.

It has given renewed prominence to particular lines of cleavage and granted legitimacy to particular factional claims. It has helped to rationalize the avant-garde in social research. It would be wrong, however, to assume that the presence of an avant-garde is an entirely recent phenomenon or entirely dependent on appeals to the postmodern.

There are, then, tendencies to make the ethnographic enterprise increasingly problematic. We shall discuss those tendencies in more detail later in this chapter. We contrast them with a countervailing tendency in contemporary ethnographic research. Alongside the centrifugal, fragmentary movement we have just alluded to, there is a centripetal tendency. This too can be seen in terms of the cultural diversity or contestation inherent in ethnography. We identify a convergence, endorsed by some qualitative researchers and methodologists, towards an ideal-type of data collection, storage and analysis. This model combines computing techniques with methodological perspectives claimed to be associated with 'grounded theory'. One can detect a trend towards a homogenization, and the emergence of a new form of orthodoxy, especially at the level of data management. We note that the use of microcomputing strategies for qualitative data handling has become wide-spread, and this includes an almost globalizing process within the research community. The presuppositions and procedures that are inscribed in contemporary software for qualitative data analysis are implicitly driving a renewed orthodoxy that is being adopted in a large number of research sites around the world. We deal with these issues in order to illustrate our general argument concerning the internal differentiation of ethnography itself. We are not concerned here with specific and detailed methodological critique.

The remainder of this chapter expands upon the ideas and contentions we have outlined above. Before engaging with the more current debates of ethnographic diversity and representation, we give an historical context to our argument. In the following section we outline some of the ways in which ethnography has a long history of differences and discontinuities, emphasizing the cross or trans cultural positioning of the ethnographic.

ESSENTIAL TENSIONS

In their recent, and in many ways authoritative, review of the development of qualitative research traditions, Denzin & Lincoln (1994: 7ff.) write of the "five moments" of qualitative research. The first moment they identify as an objectivist and positivist programme, sustained by myths of the heroic lone fieldworker. Of researchers in this moment, spanning, 1900 to World War II, they write: "They were concerned with offering valid, reliable, and objective

interpretations in their writings" (p. 7). The second moment is described as modernist and creative, characterized *inter alia* by attempts to formalize qualitative research methods: "The modernist ethnographer and sociological participant observer attempted rigorous, qualitative studies of important social processes including deviance and social control in the classroom and society" (Denzin & Lincoln, 1994: 8). This modernist phase comes to an end in the late 1960s, superceded by a "third moment." The third moment spans 1970 to 1986, and is described by Denzin & Lincoln in terms of "blurred genres." There was, they claim, a new multiplicity of theoretical orientations and paradigms. Diverse ways of data collection and analysis also came to the fore during this moment. For instance:

> Diverse ways of collecting and analyzing empirical materials were also available, including qualitative interviewing (open-ended and quasistructured) and observational, visual, personal experience, and documentary methods. Computers were entering the situation, to be fully developed in the next decade, along with narrative, content, and semiotic methods of reading interviews and cultural texts (Denzin & Lincoln, 1994: 9).

It is to this moment that Denzin & Lincoln locate the contribution of Geertz (e.g. Geertz, 1973, 1983). Their summary evaluation of this period is: "The golden age of the social sciences was over. and a new age of blurred, interpretative genres was upon us" (p. 9)

The fourth moment coincides with the crisis of representation, in which canons of truth and method were challenged, not least through the critical examination of textual practices. As Denzin & Lincoln (1994: 10) suggest "the erosion of classic norms in anthropology (objectivism, complicity with colonialism, social life structured by fixed rituals and customs, ethnographies as monuments to a culture) was complete." The crises of this period or moment put in hazard not only the products of the ethnographer's work, but the moral and intellectual authority of ethnographers themselves.

Finally, the "fifth moment" presented by Denzin & Lincoln, is characterized by continuing diversity and a series of tensions. For instance, they write in their more developed account of this fifth moment:

> Qualitative research embraces two tensions at the same time. On the one hand, it is drawn to a broad, interpetive, postmodern, feminist, and critical sensibility. On the other hand, it can also be drawn to more narrowly defined positivist, post-positivist, humanistic and naturalistic conceptions of human experience and its analysis. (Lincoln & Denzin, 1994: 576).

We do not wish to dissent from Lincoln & Denzin in their account of the current state of play in qualitative or ethnographic research. There is no doubt as to the multiplicity of perspectives and practices which they identify. Their discussion helps us to characterize ethnography in temms of its own cultural

diversity. Indeed, in the second part of this chapter we shall address some of those tensions ourselves. We are, however, less convinced by Lincoln & Denzin's attempt to periodize the development of qualitative research, and even less convinced by the particular developmental narrative they seek to impose. It would be futile merely to criticize a text such as theirs just for simplifying things: simplification is inherent in such didactic and introductory texts – as Ludwik Fleck pointed out many years ago (Fleck, 1979). On the other hand, we suggest that their sequence of moments glosses over the persistence of tensions and differences in ethnography. Each of their periods – especially the earlier ones – is too neatly packaged. The contrast between previous positivist, modernist and self-confident (but narrow) perspectives, and the contemporary carnivalesque diversity of standpoints, methods and representations, is too sharply drawn. It does something of a disservice to earlier generations of ethnographers, while suggesting that all contemporary research takes place in an intellectual field teeming with contested ideas.

By addressing a number of selected themes here, we want to suggest that there is greater continuity in the practical accomplishment and epistemological underpinnings of ethnography than Denzin & Lincoln allow. That continuity is not one of unbroken adherence to a single orthodoxy. Rather, there are tensions and differences that are themselves recurrent. It is as wrong to assume that all ethnography in past generations was conducted under the auspices of a positivistic and totalizing gaze as it is to imply that we are all postmodern now. There is a repeated dialectic between centripetal forces – tending towards convergence on a dominant orthodoxy – and centrifugal forces that promote difference and diversity. Rather than the temporal metaphor of 'moments', the more appropriate one might be that of 'vectors', implying the directionality of forces in an intellectual field. There is, for instance, little need to appeal only to recent developments in ethnographic writing and commentary as evidence of 'blurred genres'. Relationships between the aesthetic and the scientific, or between the 'positivist' and 'interpretivist' have been detectable for many years – indeed, throughout the development of ethnographic research this century. (Admittedly, they have not been equally remarked on, nor taken the same form at all times.)

It is a well-known aspect of the history of sociology – but it bears repetition in this context – that the early period of urban ethnography in Chicago drew-on aesthetic and literary models as much as on models of 'scientific' research. In her excellent book on the subject Cappetti (1993) traces the linkages between the sociological imagination that flourished among members of the Chicago School, and the contemporaneous literary traditions. The sociological perspective was fuelled by the textual conventions of realist fiction. The

sociological celebration of the 'life' – through the life-history – was influenced by the novel of development. Equally, some of the literary inspirations drew – broadly speaking – on a sociological perspective. More generally still, the ethnographic tradition in the United States and literary genres have displayed intertextual relationships over many decades. The styles of urban realism, the literary creation of characters and types in the city, and the narrative imperative of modern fiction – these have all contributed to styles of ethnographic representation. The systematic analysis of these intertextual relations may be a fairly recent preoccupation (for example Van Maanen, 1988; Atkinson, 1990), but the genres were more 'blurred' than the simple developmental model from Denzin & Lincoln would suggest.

Indeed, the nature of those intertextual linkages deserves much closer attention. It is clearly insufficient to deal with a monolithic 'ethnography' on the one hand and an equally undifferentiated 'literature' on the other. The specific relationships between American fiction and ethnographic reportage are but one set of possible homologies and influences. Stocking (1974) and Clifford (1988) have both drawn attention to the significant parallels between Malinowski's ethnographic enterprise and Joseph Conrad's literary work. Handler (1983) explores the multiple cultural and literary commitments that informed Edward Sapir's anthropology and his linguistics. In doing so he also reminds us that in the figure of Franz Boas himself – its founding hero – American cultural anthropology was born out of a complex mix of epistemological and aesthetic commitments. Equally, Ruth Benedict's particular development of one strand of Boasian anthropology was hardly conceived and reported in a narrowly scientistic manner (Benedict, 1934)

The intertextual relations between realist literature and ethnographic work by no means exhausts the complex relationships between social science and aesthetics. Among the notable – if exceptional – examples is the work of the French anthropologist Leiris (see Clifford, 1988) The anthropological project here is linked directly with various avant-garde intellectual movements – including surrealism. Standing in stark contrast with the North American tradition of realist textual practice, this French standpoint engages anthropology, directly and indirectly, with some of the most significant currents of twentieth-century culture: but not through a naively scientistic route. Clifford (1988) has reviewed the surrealist aspects of French ethnography, and it is not necessary to recapitulate it entirely here. He reminds us that the 'ethnography' that is explored here is not the same as the empirical fieldwork of British social anthropology or American cultural anthropology. Were it so, of course there would be little to remark on. Rather, he reminds us of the intellectual currents

that linked ethnography, art, and literature. The links between the anthro-
pologist Marcel Mauss and the essayist and pornographer Georges Bataille
reveal a tendency in anthropological thought that transgresses the boundaries of
a narrowly conceived social science. Likewise, the work of Leiris represents a
refusal of the colonializing gaze rather than its celebration (Clifford, 1988:
165–174). They inspired cultural exploration that was grounded in juxtaposi-
tion rather than synthesis: as Clifford suggests, the trope of *collage* represents
the approach rather than the totalizing synthesis of more conventional
ethnographic production. Bateson's groundbreaking ethnography *Naven* (1958)
also deserves attention in this context: an early attempt to produce a text that
inscribed the 'collage' or 'palimpsest' view of culture.

Our point here is not to review yet again fairly well known commentaries on
ethnography, literature and the aesthetic. Rather, we emphasize the extent to
which ethnography, in sociology or anthropology – whether conceived in terms
of method or its textual products – has never been a stable entity. It has been
marked by contrasts and tensions that are not merely departures from an
established orthodoxy. The conduct of ethnographic research has rarely, if ever,
been established solely under the auspices of a positivist orthodoxy. American
cultural anthropology, for instance, has displayed a repeated tension between
'nomothetic' search for law-like regularities, and the 'idiographic' inter-
pretation of specific cultures. Furthermore, given the highly personalized
nature of anthropological fieldwork and authorship, it is far from clear that any
major practitioner ever subscribed to a purely scientistic or positivist
perspective. Indeed, although it is virtually impossible to demonstrate, one
suspects that the social and academic elite members of the community of
anthropologists (in Britain at any rate) never subscribed to anything quite as
vulgar or artisanal as a single 'scientific method' or its equivalent. The
emphasis on personal qualities and the uniquely biographical experience of
overseas fieldwork meant that the discipline of anthropology was often
portrayed as an essentially 'indeterminate' mode of knowledge acquisition.

In other words, for all the tidying up of accounts like that of Denzin &
Lincoln, ethnographic research has always contained within it a variety of
perspectives. As a whole it has never been totally subsumed within a
framework of orthodoxy and objectivism. There have been varieties of aesthetic
and interpretative standpoint throughout nearly a century of development and
change. The ethnographic approach to understanding cultural difference has
itself incorporated a diversit of intellectual cultures. To be sure, there have been
changing intellectual fashions and emphases, and the pace of change has
undoubtedly been especially rapid in recent years. But they reflect long-
standing tensions, rather than constituting a new and unique 'moment' in

ethnographic research. They continue the centrifugal and centripetal tendencies that have been perceptible for many decades.

In the sections that follow we shall comment in more detail on some of the subcultural differences and contestations within contemporary ethnography. We begin by considering the the general issue of ethnographic representation. Representation has been the origin of a great deal of the centrifugal, fragmenting tendency within the intellectual field. It provides a contempoary context in which ethnography can be seen as promoting a diverse spectrum of approaches, being located at a crossroads of crises and experimentation.

CULTURAL CONTESTATION AND THE REPRESENTATION OF CULTURE

Contemporary ethnography encompasses a variety of perspectives and contestations, none less so than in the area of representation. The centrifugal tendencies in contemporary ethnography include contested issues of representation. It is in this domain – as in earlier decades – that the *avant-garde* in ethnographic cultures is most clearly visible. Contemporary debates over the ethnographic representation of cultural phenomena have concentrated on the textual construction of reality (Atkinson, 1990, 1996). Emerging most starkly within the discipline of anthropology, such debates have now spread to sociology and the ethnographic endeavour more generally. At the centre of such debates is the critical appraisal of ethnographic writing and the social production of the ethnographic text. Traditionally. the professional and academic status passage has been completed and confirmed by the construction of a major text. The anthropological monograph, therefore, was the culmination of the ethnography and the legitimating mark of the anthropologist (see Coffey & Atkinson, 1996 for U.K. data on the continuing significance of 'fieldwork'). The relationships between fieldwork, text production and the discipline of anthropology, have then developed over time. The anthropologist was identified with his or her people, who in turn were identified with and in the ethnography. The ethnographic monograph thus became the embodiment of the discipline itself and the identify of its practitioners. Within the classical period of British and American anthropology the ethnographic monograph enshrined a series of standardized representations of societies and (by implication) of their authors (Boon, 1982). There are, of course, other modes of ethnographic representation, historically but not so widely used, including film. They are as conventional and artful as any written text (cf Crawford & Turton, 1992; Loizos, 1993).

Given the importance of the ethnography as textual product it is a little wonder that radical assaults on its status should strike at the roots of the discipline. Thus in recent years, anthropology – once so stable – has experienced a 'crisis of representation'. The textual foundations have been shaken and, along with them, the intellectual faith that has informed their production and reception. The status of ethnographic texts has also come under scrutiny from within sociology (Atkinson, 1990, 1992; Hammersley, 1992). In many ways this has proved a less critical issue for sociology than for anthropology, not least because ethnographic methods and monographs are much less central to sociology as a whole. Important though qualitative research is in many fields of empirical sociology, it does not underpin the entire academic enterprise as it does for anthropology. The critiques of ethnography in sociology have sometimes followed directions similar to those in anthropology, (see Hastrup, 1992; Richardson, 1994). Several of the positions which have contributed to such critiques have been associated with the general thrust of postmodernism. Postmodernism in general has certainly contributed to reappraisals of cultural representation, in the human sciences and beyond. It should also be acknowledged that recent developments in ethnographic representation are not dependent on postmodernism *per se*. Man of the current tendencies can be understood as developments of anthropology and socio-logical perspectives, rather than radical departures from them. Indeed, as we have suggested already, they reflect an inherent tension in the ethnographic project – between the 'scientific' and the 'aesthetic'. It is possible to identify a number of contributions to the contestation of ethnographic representation, and a number of trends which have emerged out of such claims. For the purposes of this chapter we concentrate on just some of these.

The weakening of cultural (and indeed) disciplinary boundaries and the consequent cultural proliferation of ethnographic work have been spurred by a movement which we might usefully call the 'rediscovery of rhetoric'. Rhetoric is no longer consigned to the margins of legitimate scholarship. It has more recently been recognized as central to scholarly work and production. The classical theory and practice of rhetoric was concerned with argumentation and persuasion. The separation of rhetoric and science at the Enlightenment implied a radical distinction between two contrasted sets of commitments. On the one side stand together science, reason, logic, methods and evidence. On the other side are ranged rhetoric, persuasion, opinion and ornamentation. The aspirations of modern scholarship were firmly rooted in such dualities. The separation of rhetoric from logic in the creation of modern disciplinary knowledge parallels a number of other. equally fundamental, separations and dichotomies. It established the possibility of an observer armed with a neutral

language of observation (since untouched with rhetoric) and thus allowed for
the elementary distinction between that observer and the observed. The
rediscovery of rhetoric creates the possibility of removing such distinctions in
ethnographic textual practice. It promotes the removal of distance between the
subjects and objects of inquiry, and questions taken-for-granted canons of
science and reason. It reminds us that scientific accounts and texts have
rhetorical qualities. It challenges cherished distinctions between scientific fact
and textual production, or between the reality of the natural-scientific world
and narrative accounts of the social world. (For other accounts of representa-
tion of the natural and social, see: (Lutz & Collins, 1993; Bazerman, 1988;
Myers, 1990; Lynch & Woolgar, 1988).

The work of authors such as Edward Said (1978) has placed this weakening
of cultural boundaries more overtly within an ideological perspective. Said's
sustained commentary of the *orientalism* of western observation has served to
strengthen the case that traditional ethnographic texts have a privileging effect.
That is, the cultures which have been represented have been reduced to the
subjugated and muted objects of a dominating discourse. In enumerating and
classifying the exotic characteristics of the oriental, then, the privileged
observer has established a position of authority, which is inscribed in the texts
of exploration, description and classification (Marcus, 1992). A virtually
identical set of issues can be described for the encounters of the old world with
the new in the conquest and appropriation of the Americas. From the earliest
accounts of the Spanish conquests through to the accounts of nineteenth-
century explorers and ethnologists, the continent has been populated by others
and appropriated through the accompanying representations. The texts of
exploration and exploitation repeatedly inscribe the metropolitan perspective
and the alterity (otherness) of the new world (Pratt, 1992; Todorov, 1984).

Feminist theory and praxis has also questioned the thus far privileged
position of observer-author. Here the argument has not been about the over-or-
under representation of men and women as ethnographic authors. but rather
about the relationships between feminism, gender and ethnography at more
fundamental levels. Clough (1992), for example, argues that from a feminist
standpoint one can see the standard realist accounts of ethnography as
incorporating unconscious fantasies and desire concerning race, gender and
class. Realism, she argues, suppresses those unconscious processes under the
guise of factual discourse Wolf (1999) also addresses the feminist perspective
on ethnography and representation. She suggests that reflexive, self-critical
attitudes are particularly characteristic of feminist thought. Feminism, in
general, encourages an examination of power and powerlessness, the mutual
obligations of researcher and researched. She implies that feminist scholars

were exploring these issues independently of their becoming fashionable topics among male anthropologists. As Wolf also suggests, the heightened sensibilities of feminist scholars have led directly towards problems of representation. Mascia-Lees et al. (1989) also draw attention to this, arguing that feminist anthropologists have long called for modes of understanding (including writing) that do not reduce women to the position of voiceless *objects*, but treat them as subjects in their own right, entitled to their own voices. This echoes the very foundations of the feminist research process – the concern with voice and authority, accounts and experience (Smith, 1987; Olesen, 1994). This feminist strain of ethnographic critique is reminiscent of the distinction, first elaborated by Shirley and Edwin Ardener (e.g. Ardener, 1975), between dominant and muted groups; the concern that some groups (the muted) are deprived of a culturally legitimated means of expression, visible and audible only through the eyes, voices and texts of dominating groups. As a consequence, they cease to be the subjects of their own experiences and actions; they are reduced to being, the objects of other subjects. They are subjugated in that sense. It is argued by feminists and other critics of classic ethnographic discourse that the 'others' of such inquiry and such description are rendered mute. Indeed, when the objects of ethnography are already dominated (as are women, for instance) the ethnographic gaze may be in danger of performing a kind of double subjugation.

The proper representation of social reality has therefore become contested from a variety of perspectives and standpoints. One major consequence of this has been critical focus on the production and reading of ethnographic texts. The historical and stylistic continuities with so-called realist fiction have been well documented (Krieger, 1983, 1984; Atkinson, 1992; Cappetti, 1993) and we have already discussed some of them. Literary realism has been identified as the dominant mode of representation, implying an impersonal, all-but invisible-narrator (Van Maanen, 1988). It is presented from the point of view of one impartial author. His or her point of view is the dominant, even the sole, one. It is a genre of authoritative reportage. As a style, as a collection of literary devices, such realist writing is a massively familiar one for the construction of factual authoritative accounts. There is, therefore, the danger of taking it for granted, and hence of treating it as a natural way of representing the social. Despite this tendency towards a realist approach, it remains by no means clear that literary realism is the only – or even the best – way to produce accounts of varied social worlds. Indeed, as Atkinson (1983) noted, there is something of a paradox in the use of what one might call a "straightforward" realism for ethnographic purposes. There is a tension between the conventions of realism and the assumption of most ethnographic work. For most ethnographers –

whether sociology or anthropology be their primary discipline – recognize the complexity of social life and its collective representations. Equally, they recognize the fundamentally constitutive nature of language. That is, language use creates and constructs social reality. Interpretative anthropologists, for instance, are committed to the ideals of 'thick description', while symbolic interactionists equally endorse an interpretative sociology that places language at the heart of an essentially constructivist view of reality and representation. And yet conventional realism is founded on a very different treatment of language. Such realism has historically encouraged little or no explicit concern for the language of representation itself. Realism treats language as a taken-for-granted resource. The realism of conventional writing may therefore result in 'thin' description. Such arguments – that narratives and descriptions from a single, implicit point of view may not do justice to the complexity of cultural forms – have given rise to various alternative approaches.

These alternative approaches epitomize the diversity of more recent ethnographic work and reflect the interpretative turn in ethnographic writing and representation. Various commentators have called for texts that are more open, messy and fragmented in order to do at least two things: firstly to challenge and highlight the very conventionality of such ethnographic writing. Secondly to allow for more creative and complex modes of representation (cf Mulkay, 1985). While the conventionality of all modes of representation is recognized, there is more than a hint in such arguments that complex texts may be more faithful to the complexities and contours of social life. We have discussed some of these alternative forms of representation elsewhere (Atkinson & Coffey, 1995, Coffey & Atkinson, 1996) and so will not recapitulate these discussions here. The sorts of alternative representational modes we have in mind are exemplified in the recent edited collection on *Composing Ethnography* (Ellis & Bochner, 1996) and would include the dialogic approach, (Dwyer, 1977, 1979; Allan, 1994; Holquist, 1990); ethno-drama or ethno-theatre (Pages, 1990; Mienczakowski, 1996); poetry (Richardson, 1992; Austin, 1996), diaries and (auto)biographical texts (Foltz & Griffin, 1996; Tillmann-Healy, 1996). These approaches are closely related to the promotion of biographical and autobiographical work in anthropology and sociology more generally, in particular, on the 'writing' of lives and selves (Stanley, 1992; Hastrup, 1992). For a recent overview of alternative representational forms see Ely, Vinz, Downing & Anzul (1997).

The general affinities between experimental ethnographic writing and postmodernism are clear. Postmodernism, in recognizing and celebrating the diversity of types and representations, encourages a variety of genres. It also encourages the blurring and mixing of genres. It questions the monovocal

expression of authenticity in favour of polyvocal texts and the celebration of diversity. There is, therefore, much in the postmodernist movement to commend various radical re-evaluations of ethnographic writing. On the other hand, it is not necessary to appeal to postmodernism. It is arguable that the possibilities for textual experimentation are contained within the modernist movement in literature. Modern literature provides us with a multiplicity of textual formats and devices for the construction of written representations. Modernist fiction found many ways, for instance, of representing the mingling of external events and inner dialogue; of reconstructing the minutiae of extraordinarily detailed description; of linking factual reportage with the fantastic. By adopting some of these 'new' conventions, and by experimenting in similar ways, ethnographic texts can also be viewed as undergoing a modernist movement. It is our contention, indeed, that contemporary appeals to postmodernism per se in this context exaggerate the recent cultural tensions and diversities, and equally exaggerate supposed epistemological breaks with past positions

Having prosided an overview of representational issues we now turn to a further area of contemporary ethnographic practice. We identify a trend toward what we term a 'computing moment'; emphasizing that for the purposes of this chapter we are less concerned with an evaluation of particular computer software as with the more general issues of competing 'orthodoxies' in ethnography.

DATA ANALYSIS, GROUNDED THEORY AND THE COMPUTING MOMENT

We have identified a diversity of approaches to ethnography and representation. The interpenetration of the scientific and the rhetorical, and the contestation of representation makes ethnography itself a contested cultural practice. The contemporary currents that we have discussed continue and amplify long-standing fissiparous tendencies. It is also possible to identify a different trend – towards a form of homogeneity in ethnographic practice The progressive codification of ethnographic method provides an equally long-standing trend in the opposite direction. An emerging orthodoxy is being adopted globally by key members of the qualitative research community. This is frequently, but not exclusively, linked to versions of "grounded theory" (Glaser & Strauss, 1967; Strauss & Corbin, 1990, 1994; Glaser, 1978; Charmaz, 1983). This is itself a site for the interplay between competing intellectual cultures. Again, it is not our purpose here to review the different interpretations and uses of grounded theory, nor to rehearse the particularities of the dispute between its progenitors.

Rather, we use it once more to illustrate the essential tensions within the broad intellectual field of ethnographic research, and the work of cultural interpretation. (The varieties and ambiguities of interpretation surrounding such methodological ideas need more thorough exploration that we can do justice to here).

Grounded theorizing has been received in two ways – sometimes quite starkly contrasted. On the one hand, it may be read in terms of a general strategy of social inquiry – only loosely tied to any particular 'method' of data collection and analysis. The ideas reflect a broadly pragmatist philosophy of science spanning from American philosophers of science such as Peirce to Mead and Dewey (cf. Strauss & Corbin, 1994: 279 ff.). It represents research as a process of transactions with the natural or social world; it stresses the practical nature of inquiry, and it supposes that truth is "enacted" (cf Addelson, 1990). The methodological precepts outlined by Glaser & Strauss (1967) in their original publication are therefore seen as strategic and heuristic. They provide generalized descriptions and guidelines concerning the researcher's engagement with the world under investigation, her or his data, and ideas or theories. It is a quite explicit rejection of a positivist epistemology, and in its original form, it resists translation into simple formulae and prescriptions.

That reading of grounded theory has not prevailed in all contexts. On the contrary, a great deal of empirical work, and secondary methodological writing, have translated it into 'a method' that can be reduced to prescriptive recipe-knowledge. The methodological literature to which Strauss himself contributed helped to construct that reading to some extent. The process of simplification that is attendant on the drawing up of recipe-type procedures removes grounded theorizing from a realm of generalization and creative exploration, toward a much more bounded understanding of the concepts and ideas.

This is partly linked to the growth of Computer Assisted Qualitative Data Analysis Software (CAQDAS) as a subfield of 'methods' expertise (Lee & Fielding, 1991). Software packages aimed at analysing qualitative data are now widespread and it is a fast growing field. We do not intend to review all of that literature, or all of the existing software. That has been done elsewhere (Burgess, 1995; Tesch, 1990; Weaver & Atkinson, 1994; Weitzman & Miles, 1994; Coffey & Atkinson, 1996; Coffey, Holbrook & Atkinson, 1996). We note in particular the conergence of most computer applications on a general model of data marking and retrieval, and on one particular model of grounded theorizing. Many of the software packages may most accurately be described as computer-based applications for the storage and retrieval of data. While

there are additional facilities and sophistication involed, the general notion of
coding remains fundamental to such CAQDAS.

We recognize that qualitative analysis software does not in itself prescribe a
particular epistemological or methodological grounding. Equally, software
does not constrain its uses (Fielding & Lee, 1995). It is open to researchers to
use such software in as many different ways as they see fit. It is also important
to distinguish the intentions and advice of developers from the everyday uses
to which computing is put in practice. It is, however, discernible, that the
practical application of computing software for qualitative data analysis has
often been based on a closely related set of assumptions. They are: grounded
theory depends on an inductive approach to analysis; it rests on the coding of
data, coding consists of data reduction into categories; analysis depends on the
retrieval of coded segments; a search-and-retrieve strategy is therefore the
implementation of grounded-theory inquiry. None of those propositions is
uncontested, and we do not agree with all or indeed any one of them in such
a simple sense. They do not entail one another. Nevertheless, there is ample
evidence – especially from applied fields such as educational or health research
– to suggest that there is an emergent orthodoxy along those lines. A similar
view of the convergence of 'grounded theory' (in crude over-simplifications)
and CAQDAS has been outlined by Lonkila (1995). Lonkila suggests that
aspects of grounded theory have been overemphasized in the development and
use of qualitative data analysis software, while other approaches have been
neglected in comparison. There is, therefore, a danger that researchers may be
led implicitly towards the uncritical adoption of a particular set of strategies as
a consequence of adopting computer-aided analysis. The emphasis on coding
data is a central feature of this process of convergence. Grounded theorizing is
more than coding, and software can be used to do more than code-and-retrieve
textual data. The point here is not about the full potential of CAQDAS, nor
about the true nature of grounded theorizing. Rather, the danger lies in the glib
association between the two, linked by an emphasis on data coding
procedures.

It is too easy for there to develop a taken-for-granted mode of data handling.
This is not necessarily an inherent feature of software itself: it resides in the
uses to which such software is put. In our view, the association of CAQDAS
with an over-simplified 'grounded theory' justification can be misleading to
students and researchers to whom it is introduced. CAQDAS offers a variety of
useful ways of organizing data in order to search them, but coding data for use
with computer programs is not *analysis*. It is important to avoid the
misapprehension that coding and computing lend a scientific gloss to
qualitative research. The growing 'respectability' of qualitative methods,

together with an adherence to canons of rigour associated primarily with other research traditions, can lead to the imposition of spurious standards (Fielding & Lee, 1995). The categorisation of textual data and the use of computer software to search for them appear to render the general approach akin to standardized survey or experimental design procedures. Elsewhere (Coffey, Atkinson & Holbrook, 1996; Coffey & Atkinson, 1996) we have argued that qualitative research is not enhanced by poor imitations of other research styles and traditions. Analytic procedures which appear rooted in standardized, often mechanistic procedures are no substitute for genuinely 'grounded' engagement with the data throughout the whole of the research process. It is worth noting that the 'usefulness' of such computer programmes imply that you have collected and inputted all of your data, and this suggests that data collection and data analysis are discrete and linear.

There are now various evaluations of the methodological and practical value of various software applications. The general approach has spawned its own area of expertise. (See Tesch, 1990; Weaver & Atkinson, 1994, 1995; Weitzman & Miles, 1994; Fielding & Lee, 1995; Burgess, 1995). As a consequence of that, there is an increasing danger of seeing coding data segments as an analytic strategy in its own right, and of seeing such an approach as the analytic strategy of choice. Coding segments of text, with or without the addition of analytic memoranda to selected segments, is by no means the only way of managing and manipulating data. It is not even the only way of thinking about the use of computers for qualitative data analysis (cf Weaver & Atkinson, 1994; Coffey & Atkinson, 1996; Coffey, Holbrrok & Atkinson, 1996).

This constellation of research approaches, now has its own small-scale industry of software developers, marketing strategies, training courses and advocates. It is an emergent field of specialization among qualitative researchers. Or to put it another way, in keeping with the more general approach of this chapter, there is a sub-culture of qualitative research generally (and ethnographic practice in particular) that lends itself to this way of thinking about and representing data.

CONCLUSION

In this chapter we have attempted to demonstrate the ways in which ethnography stands at a cultural crossroads. Moreover we emphasize that the diversifications and contestations are not new to ethnographic practice. Rather such continuities and discontinuities are part and parcel of what actually constitutes ethnography itself.

While there are centrifugal tendencies within contemporary ethnography, pushing various 'alternative' strategies, celebrating 'alternative' representations, and creating a self-conscious *avant garde* among sociologists, anthropologists and other cultural disciplines, one can readily detect a countervailing tendency. The drive to codify data analytic techniques – with or without the support of computer software – coupled with appeals to standardized methodic procedures and appeals to grounded theorizing is a potent one. We repeat that we do not believe such perspectives to be inherent in computer software: clearly software does not totally constrain its use. We have ourselves suggested some ways in which contemporary information technology can serve a diversity of research interests (Coffey & Atkinson, 1996; Coffey, Holbrook & Atkinson, 1996). In practice, however, there are clearly pressures towards a codification and technicization of analytic procedures that run counter to the carnivalesque proliferation of alternative strategies.

We have not attempted to undertake a comprehensive review of the current state of the art. The intellectual field of ethnography, and of qualitative research methods more generally, is too vast for such an exercise to be undertaken in one chapter. Our intention has been to emphasize the cross-cultural nature of ethnography itself. It has repeatedly been characterized by contrasting, sometimes competing, intellectual currents. They themselves reflect cultural diversity. Ethnography draws on aesthetic as well as more 'scientific' criteria and inspiration, drawing on literary and other models in the humanities. The tension between the idiographic and interpretative on the one hand, and the nomothetic and positivist on the other, is a recurrent one. The cultural tensions are marked in the current 'moment' (to use Denzin & Lincoln once more), but have not been confined to any one developmental period.

If we cannot and should not regard 'cultures' as given entities that await our investigative gaze, then equally we cannot regard the ethnographic enterprise as a homogeneous academic culture itself. We draw on an ethnographic *bricolage* in order to explore the diverse practices of 'other' cultures and to defamiliarize our 'own'.

NOTES

1. The goddess Hecate was associated with crossroads, and was sometimes called Trivia in Latin, the name referring to the meeting of three ways. Out title therefore invokes the goddess as we argue that ethnographic research itself – its methods and representations – is to be located where various cultural roads meet. In itself, therefore, ethnography is 'cross-cultural'.

2. An earlier version of this paper was presented at the ESRC seminar series from which this collection is derived. This is considerably revised. The earlier paper has since been published elsewhere (Coffey, Holbrook & Atkinson, 1996). We are grateful to the seminar participants for their comments, which have informed some of our subsequent revisions.

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SOME ISSUES AND PROBLEMS IN CROSS-CULTURAL CASE STUDY RESEARCH

Robert G. Burgess

Editors' Commentary: *Burgess begins this third chapter by posing questions about the implications of globalisation for social research and the need to find solutions to 'new' methodological problems which this might pose. Developing the work of Yin (1989) and Platt (1988), the chapter considers the definition of a case study by inviting researchers to consider the intellectual, temporal and methodological boundaries of the case that constitutes the case study. Drawing on team based case study research into postgraduate education and training, conducted across seven countries, the chapter stresses the need for agreement amongst team members over units of study, principles of selection and definitions of key organizing concepts. Burgess' approach to cross-cultural case study sees the possibility of utilizing a wide range of research tools as a way of addressing issues of reliability and validity in social research.*

With the development of globalisation and the promotion of cross-cultural research in the social sciences, we are likely to have to solve 'new' methodological problems. Many of these issues will be 'new' given that they will demand a knowledge of issues and problems concerned with comparative cross-cultural research, much of which will take place within contrasting societies in Europe and elsewhere. Yet some people may argue that nothing is 'new' as many anthropological studies have drawn on cross-cultural data.

However, a recent trend has witnessed social and educational studies drawing on cross cultural data from different European societies. The bulk of these studies have been conducted at a macro level on a country-wide basis using historical data (Archer, 1979), or alternatively macro studies that have drawn on quantitative data in reports produced on behalf of the OECD (Skilbeck, 1994). It is, therefore, the purpose of this chapter to explore some of the 'new' methodological issues that arise in the context of using and conducting case study research in a European context. To illustrate some of these issues and problems, reference will be made to a project concerned with postgraduate education and training that has been conducted in seven European societies (Burgess, 1993a, 1999).

A DEFINITION OF CASE STUDY RESEARCH

Many writers have pointed to the problems involved in defining what constitutes a case study (Mitchell, 1983; Platt, 1988; Schuller, 1988). As these researchers have pointed out, the term 'case study' is widely used and mis-used in the social sciences given that many people suggest that case study work is qualitatively based, and focused on subjective data. Often the life history is quoted as a key example of this kind of work. Yet nothing could be further from the situation in which case study investigation is based. Indeed, there are a range of qualitative and quantitative methods of investigation that are used in the context of conducting case study. For many writers, the situation is not so much one of defining what constitutes a case study, but rather what constitutes the case as it is the unit of analysis that is crucial to case study.

A further aspect of the mythology of case study is illustrated by the fact that some writers suggest case studies can be identified through the use of a single instance when studying one factory, one hospital, one school, or one event. However, this is to restrict the field of enquiry far too much, as several investigators have demonstrated that case study research can involve multiple cases as well as the single case (Stenhouse, 1984; Burgess et al., 1994). However even when using multiple cases, it is essential that issues of comparison are addressed. In that sense, comparisons have to be examined in the context of the particular study in order to decide what would constitute sampling and what the purpose of conducting a series of connected cases might be.

This bring us to another element of case study research, namely the time period over which case studies can be examined. In some senses, this involves thinking about the links that can be made between the contemporary unit of study and the historical unit of analysis. Furthermore, researchers need to

consider the extent to which the unit of analysis needs to be defined and boundaries established in order that the 'critical case'; that is, a particular unit of study, can be examined in the course of conducting a piece of research (Burgess, 1993b).

Already, we have seen that there are problems associated with the idea of working on a case study without taking account of the social context within which one is working. Among the issues that are placed on the researcher's agenda are the definition of the case itself, the extent to which a single case or multiple cases are used, the extent to which the case can be defined in terms of boundaries, the time period over which the case is examined, and whether it is examined in an historical or contemporary context. However, important to all these issues of research design is the matter of research technique. There is no question of saying that the case study is defined in terms of methods of investigation. Instead, as in other aspects of social investigation, the researcher needs to tackle the research problem with as wide a range of methodological tools that are available in relation to the research problem, as issues of reliability and validity have to be addressed as in all social research (Shipman, 1988). In turn, questions need to be asked about the extent to which an investigator can begin to delimit what constitutes 'doing case study research'. In part, this has been addressed in a classic account of case study research by Robert Yin when he states that a case study is an enquiry that:

• investigates a contemporary phenomenon within its real life context; when
• the boundaries between phenomenon and context are not clearly evident; and in which
• multiple sources of evidence are used

(Yin, 1989: 23).

Here, two major issues are brought out. First, that the case study allows the individual to study a particular phenomenon in situ. Secondly, that case study research involves the researcher using a range of data (often acquired by different methods of investigation). However, two further issues are also implicit. First, that the case study does not rest merely upon a single method of investigation. Secondly, that a case study in terms of a research strategy, can be differentiated from the other major methods of investigation (de Vaus, 1986). In this respect, the case study approach can be specified as being separate from research strategies such as experiments, surveys and historical methods (Burgess, 1994a).

At this point, we can begin to explore Jennifer Platt's definition of case study research as in my view it takes us further in relation to the principles of research design, and also in terms of embracing comparative and multi-site

case study. Platt suggests that we can begin to pool a whole range of ideas when she states:

> For our purposes here, a 'case study' is one where more than one case may be used, but if more than one is used the individuality of each case is retained and/or the *number* of cases falling into a given category is not treated as of significance (note that this definition subsumes most versions of 'comparative method'. The distinction between designs where comparison is internal to the study and those where it draws on other work is not of logical importance). The unit case may be a person, a small group, a community, an event or an episode. (Platt, 1988: 2).

Here, Platt points to the importance of units of study given that this aspect of case study research is fundamental to the methodologist and the researcher being able to define what constitutes 'case study'. But here we need to explore the issue of units of study, points of comparison and the use of multi-site cases through the study of postgraduate education and training in a range of European countries.

STUDYING POSTGRADUATE EDUCATION AND TRAINING

Postgraduate education and training in Europe and world wide shares a set of common issues and problems. Burgess & Schratz (1994) have identified a number of key issues that were on the agenda of politicians, policy-makers and practitioners across a range of European and non-European societies in the mid-1990s. These include:

- The role of education *versus* training.
- Academic *versus* vocational purposes of postgraduate education.
- Organisation structures; especially graduate schools.
- Selection of students.
- Supervisory practice.
- Monitoring and evaluation.
- Time to Degree.
- Submission and completion rates.

While these issues are common to a range of societies, they are not easily handled from a methodological point of view. Many of the studies that have examined these trends and developments have been conducted at the level of a country, and focus on the policy process (Burgess, 1994b). However, if we are to understand the micro-processes associated with these phenomenon, it becomes essential to focus on the institutional level. In these circumstances, it becomes possible to examine the *actual* practice and to focus upon the

processes and products associated with postgraduate education and training. Indeed, in the U.K. the ESRC has sponsored a number of studies on postgraduate education in the social sciences (Burgess, 1994c) which include a series of case studies of academic disciplines and the different ways in which postgraduate training takes place in different disciplines and in different intellectual and social contexts (Burgess, Pole & Hockey, 1994; Hill, Black & Acker, 1994; Parry, Atkinson & Delamont, 1994). In each project, the investigators utilised a multi-site case study approach to examine the processes that occurred within postgraduate training in the social sciences. However, the initiative of which this was a part also involved a number of quantitative studies which complemented the qualitative case study work that had taken place (Connor, 1994; Bulmer et al., 1994). In its ideal, a series of studies that address issues in postgraduate education and training need to draw on both qualitative and quantitative data by combining qualitative and quantitative approaches within case studies.

CONDUCTING CASE STUDY RESEARCH ON POSTGRADUATE EDUCATION AND TRAINING IN EUROPE

The study of postgraduate education and training in seven European societies was designed to draw on the experience of a series of interdisciplinary teams. At one level, this may be seen as an advantage in developing social scientific work. However, as Platt (1988) has indicated, in the context of conducting case studies, this may be problematic given that different disciplines envisage different ways in which case study work can be conducted. For example, sociologists have talked about case studies being conducted through the use of methods such as participant observation (Becker, 1968), while political scientists have focused on comparative studies (Lijphart, 1971; Epstein, 1975). In contrast, educational researchers have discussed case study as an alternative methodology to experiments and surveys (Simons, 1980), while psychologists have taken an eclectic approach as demonstrated by Yin (1989). Finally, anthropologists, such as Mitchell (1983) have taken a sequence of events as constituting a series of interconnected cases that are the basic data to be used during the course of conducting case study investigation. Accordingly, bringing together a set of researchers from different disciplinary backgrounds may appear to have an advantage at one level, it was also problematic given the different conceptual understanding and different meanings that the team members brought to the investigation. In that sense, while the project involved a single team that I co-ordinated from the U.K., one could also argue that it was

no more than a series of teams that were used to gather data on postgraduate education within their own countries or countries where the researchers were familiar with the language. Our proposal suggested that we would look at doctoral studies in the fields of business studies and engineering. But herein lies a set of further problems. First, what constitutes the doctorate? Even if we only take two different societies, we find there are differences in terms of the length of training, the type of training that is given and the way in which the qualification meshes with the undergraduate degree, with postdoctoral work and future employment in higher education and elsewhere. Indeed, if we look at the context in Austria and Britain, we find that the mechanisms associated with doctoral work are different. They have a different history and impact on different developments within the studies in which we were engaged. Accordingly, the team had to examine ways in which a common unit of study or a series of units of study could be devised to be used across different societies. At one level, we could make a virtue out of difference by drawing on the differences that occur in doctoral studies and the different patterns of work within different disciplines. But it was also important to obtain conceptual clarification which allowed the researchers to study fundamental processes across different countries and different disciplines. In this sense, taking issues such as 'supervision' and 'training' meant that our team could use these concepts within the context of the different countries in which the studies were located. Furthermore, comparisons could be made using the concepts, not merely as organising features of the study, but also as units of analysis that were subject to critical scrutiny.

STRATEGIES OF CASE STUDY WORK

The Single Case Study

The single case study is probably the most common feature of social and educational research (see Ball, 1981; Burgess, 1983). In relation to post-graduate education it may involve the study of a single subject area (physics or mechanical engineering), it may involve the study of a single country (Austria as opposed to Britain), or it may involve the study of a particular university within one or other of the countries. Nevertheless, by building up a series of single cases, comparisons are possible. Single cases of countries may lead to comparison at the macro level. Single cases of subjects may facilitate cross-national comparisons within and between disciplines, whilst single cases of institutions may allow some internal comparisons to take place between what happens at faculty, and departmental levels and research group level within a

department. Yet even here there are problems, because the single case may involve a series of other cases. For example, at the level of the research group, different strategies of supervision may be detected among research team members and their studies. There are also questions to be addressed about the extent to which any of the units of study are representative. However, it is important to recall that researchers seldom conduct case studies to address situations that are representative but instead focus on social processes within a particular social context.

So how might some of these issues be handled in the context of a single case? First, it is important to be clear about the choice of research problem, and to set boundaries to that problem. Secondly, the choice of cases within a case becomes important so that the principles of selection are agreed by all team members. Thirdly, issues need to be resolved about the use of evidence and the kinds of data that can be utilised, including observational notes, transcripts of interviews, field notes and diaries. Fundamental to it all, is the control the principal investigator has over members of a research team as this will allow agreement to be reached on common units of study and common approaches being used to examine units of study. In turn, this gives the researcher some control over the case and the kinds of comparisons that can be made.

Multi-Site Case Study

One of the criticisms that is often made of the single case concerns the extent to which it is possible to generalise. In these circumstances, a set of studies can be designed that are multi-site, for example, when it is possible to look at postgraduate education and training in seven different countries. In this respect, critical cases may be selected for the purposes of comparison, or indeed multiple cases may be selected within the context of a particular country so as to facilitate comparisons within countries and beyond them. Fundamentally, selection is critical to the operation of multi-site case study so that comparisons can be made about the influence of different national policies and different social and political contexts upon the conduct of postgraduate education and training and the extent to which different higher education policies may influence the processes that take place within institutions.

Within multi-site case studies, a range of issues are raised. First, what constitutes the case? Is it the methods used or is it the data that are collected? Secondly, what problems are involved? A range of methods may be used to deliver case study data, but how can these methods be integrated with each other in order to deliver case study work. Finally, there is the problem of the quality of the data that are obtained and the extent to which comparison can be made across sites, within countries, and between countries.

   Some researchers have indicated that some of these problems can be
resolved, not merely through case study research, but also through the use of
case records. These include transcripts, field notes and observational data being
transcribed, anonymised and presented in an archive. This allows comparisons
to be conducted by a range of researchers, some of whom may have worked on
the project and others who treat the material that is available for secondary data
analysis (Stenhouse, 1978; Burgess & Rudduck, 1993). In these circumstances,
case records may be used by the researcher to construct and develop a case
study. While this is an interesting development that can be explored within the
context of comparative case study work, it is important to consider some of the
problems. First, the time availability to conduct the work; secondly, the
financial support required to produce case records before case studies; thirdly,
the time to transcribe interviews and observational data; fourthly, the extent to
which researchers working in different countries who have not collected the
case study data are able to use these data in relation to their own case study
material. Finally, there are issues to be resolved about the different theoretical
and conceptual assumptions that may be made by researchers working in
different countries, and how this may influence the data that are collected and
the ways those data are utilised within the writing process.

ISSUES FOR CONSIDERATION

Within the context of case study investigation, there are a number of problems
that need to be addressed, especially when conducting cross-site comparisons.
These issues include:

(a) What are case studies for? Here we need to think about the extent to which
    case studies are defined, selected, conducted and written.
(b) How can cross-cultural case studies be written up? Here, a number of
    issues need to be resolved. The definitional problem, the use of concepts,
    and the organising principles associated with the studies. Does each case
    study have to be similar in order for cross-site comparisons to take place?
(c) Who owns case study data? In the previous section, I indicated that
    material might be available on archive in the form of a case record that
    could subsequently be used to explore the possibility of several writers
    producing case studies drawing on the data from other workers on the
    project. But here there is the question of ownership of data and the extent
    to which the permission given to one set of researchers to use the data is
    transferable across all researchers associated with the project. In this
    respect, one of the issues that the team has to sort out is the conditions
    under which cross-cultural case study data can be collected and used.

(d) Can case study data shed light upon the relationship between policy and practice? Within the context of postgraduate education and training across seven European societies, it will be important to work at macro and micro levels. In this sense, links between macro policy and micro practice need to be made.

(e) What kinds of procedures are used in conducting cross-cultural case study research? What principles of design are involved? What methods of data collection are used? How can data analysis be conducted? And who takes responsibility for the writing process? Is it that multi-site cases conducted by different teams results in seven different cases, or is it that the single project is more than the sum of the individual case studies?

(f) What criteria can be used in defining and conceptualising a case study? Here we come back to the issue on which we started out. How do we begin to conceptualise what constitutes case study and how those case studies can be developed in a cross-cultural contest. In part this brings us to a final issue.

(g) What kind of training is required to conduct cross-cultural case study? For many of us, case studies in a European context may be conducted in terms of a researcher working within his or her own society. But to what extent do we need to train a new generation of researchers who can work cross-culturally, can bridge the conceptual and linguistic gap and can address methodological issues in the context of cross-site comparison. This is a challenge that social science researchers will need to address through future patterns of training, research and investigation.

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INTERVIEWING IN A CROSS-CULTURAL CONTEXT: SOME ISSUES FOR COMPARATIVE RESEARCH

Patricia Broadfoot

Editors' Commentary: *In Broadfoot's experience of researching issues of teacher professionalism and curriculum in England and France, cross-cultural case studies are a useful research tool. As their concern is not only with language, but also the cultural basis of meaning and understanding, they provide, she argues, the ultimate phenomenological challenge for the researcher. She warns that understanding a second language or as an alternative, ensuring effective translation services are available, may be central to cross-cultural case study but they do not guarantee understanding of social phenomena, which is dependent as much on cultural context as it is on technical language proficiency.*

THE SIGNIFICANCE OF CULTURE

Interviewing is one of the principal methodologies of social science. Along with questionnaires, observation, documentary study and discourse analysis, it is a major means of establishing participants' perspectives in a particular social setting. Although capable of varying considerably in form and purpose, all interviews share the fundamental rationale of seeking to represent with integrity the perspective of the actor or actors under study. To conduct an interview is to make the assumption that there are aspects of a phenomenon

which cannot be appreciated without recourse to the personal perspective of one or more of the actors involved in it. Further, it is likely to be the case, though not inevitably so, that the decision to interview is in turn a reflection of a commitment to a phenomenological theoretical perspective that assumes multiple subjectivities rather than one objective reality.

Inevitably, a great deal has been written about different approaches to interviewing – the advantages and disadvantages of the various approaches, 'the dos' and 'don'ts' of conducting interviews and the many ways, including computer-based software, of analysing the resultant data, see, for example, Hantrais & Mangan (1996), Øyen (1990). Much of what has been written is of a technical nature dwelling on, for example, the dangers of asking leading questions, of intimidating the respondent or on how best to record the conversation. The arguably more fundamental, if underlying issues of discourse and the mutual construction of understanding are much less often addressed in methodological texts. Yet they are crucial since both the message that the respondent aspires to communicate and the understanding of that message by the interviewer is a social construction that is heavily culture-dependent.

> Cultures has been defined by Woods (1990) as "social shared, systemic, cognitive, learned. They include values and beliefs, rules and codes of conduct and behaviour, forms of language, patterns of speech and choice of words, understandings about ways of doing things and not doing things" p. 27.

Language is a particularly important 'cultural tool' (Wertsch, 1997) which is central to the consturction and communication of meaning. It is both verbal and non-verbal – the latter a rather less well-recognised form of communication covering a constant and extremely influential repertoire of body language, gestures, tone of voice, which within a culture, are unconsciously monitored and responded to by interacting individuals. Indeed Nowicki (1997) has shown how 'dysemmic' individuals who are unable to 'read' such communication have difficulty making relationships and in operating effectively in social situations.

Any interview situation is thus only as good as the capacity of the participants to construct mutual understanding of each other's culturally-derived attempts to communicate and interpret meaning. Social and cultural meanings find expression in the codes – notably language – that individuals use in their interaction in order to convey a message. As Ingleby (1986) puts it:

> human thought, perception and action must be approached in terms of meanings p. 305

At the present time, cultures are arguably becoming both more global and more fragmented at the same time. The spread of information technology and multi-national companies as well as international travel and communication systems

has led to the development of shared 'cultural tools' across the globe. The rapid growth of English as the world's 'lingua franca' is particuarly significant in this respect. However, the post-modern age has also meant the growth and reinforcement of a range of sub-cultural identities and the corresponding breaking down of previously salient cultural categories such as nationality and gender.

All this means that interviewing in a cross-cultural context is becoming on the one hand, less problematic with the development of increasingly shared cultural tools such as the English language. On the other, the significance of more ephemeral, perhaps covert, cultural identities may render an attempt to construct mutual understanding in an interview situation subject to all kinds of subtle pitfalls. Such potential pitfalls may be associated with the affective context that the interviewer seeks to establish to provide reassurance and encouragement; it may influence the success of the informant in expressing their desired meaning or the subsequent interpretation and use that the interviewer makes of the responses elicited. In the light of these concerns, and their ubiquitous presence in any kind of social research, this chapter will explore the particular methodological issues which arise in the practice of interviewing in a cross-cultural context. It will address these in terms of the three stages of collecting, analysing and reporting interview data. The discussion draws on a series of cross-national, comparative studies in order to illustrate the issues raised. Whilst national cultural differences are clearly only one of a range of cultural contexts which might have been chosen, their scale and commonality make them particularly relevant as far as educational issues are concerned and hence a good illustration of the more general concern which is the subject of this chapter.

COMPARING CULTURES: A RESEARCH TOOL

But the theme of this chapter – interviewing in a cross-cultural context – does not only concern methodological and technical issues, important as these are. First and foremost it is concerned to highlight the distinction between those many researchers who find themselves interviewing in a different cultural context than their own – among delinquents and gangs for example, in elite public schools or among different ethnic groups and those who set out to use *comparison* per se as a source of research insights.

The problems associated with interviewing in an unfamiliar cultural environment, though potentially substantial, are not the same as those facing researchers who attempt systematic comparisons of different cultures. In the first case, the goal must be to minimise the impact of the researcher's own

culture in reporting on the phenomenon being studied. In the second, the goal is to study a given phenomenon, using methods that have been designed to elicit data which are as comparable as possible within cultures whose differences may require the adoption of significant variations in the design and/ or use of data collection instruments. The latter approach poses a number of deeply problematic issues and is extremely difficult to do well. Indeed, it may be partly for this reason that the number of such studies is relatively small.

The technical and sociological difficulties of explicitly undertaking cross-cultural studies which have already been referred to are further exacerbated by two other factors – the difficulty of gaining funding for such studies and the lack of a tradition of qualitative studies in comparative research such that the necessary theoretical and methodological base to support such a research tradition is still rather undeveloped. Crossley & Vulliamy's (1984) call for more comparative case studies has been echoed many times since, more recently for example, by Hopkins (1993), without notable success.

The explanation for this cannot be that such studies are not worthwhile. At a time of increasing globalisation, when the tendency to look to other countries and cultures for ideas about how to do things better has never been more marked (Phillips, 1989), there is no lack of interest in such studies. Rather, the explanation is likely to lie in the practical difficulties associated with the funding, execution and analysis of rigorous cross-cultural comparative studies. The more interpretive the study, the more marked such difficulties are likely to become. Moving away from the formal models of school systems and the safe terrain of policy documents more typically associated with comparative studies into the more ephemeral world of personal understanding and perception, inevitably increases the significance of the typically unarticulated assumptions rooted in cultural identity which influence the perceptions and behaviour being studied.

SOME ILLUSTRATIVE EXAMPLES

In this Chapter, some examples of comparative research studies will be used to illustrate a range of problems associated with the particular qualitative research technique of interviewing in a cross-cultural context. The first of the studies is the 1984–87 'Bristaix' study (Broadfoot et al., 1993). This was a comparative study of English and French primary school teachers' perceptions of their professional responsibilities and priorities. Part of the research involved a questionnaire study of approximately 400 teachers in each of two case-study areas – Avon in England and the broadly-matched, 'Bouches du Rhône' area of France. The aim of the questionnaire was to 'map', in a predominantly

quantitative way, the key areas of difference in the professional perspectives of the two cohorts. Another part of the study involved more detailed classroom studies in a representative sub-sample of two teachers in eight schools in each country. Both systematic and unstructured observations, as well as teacher diaries, were used in each classroom in order to explore in detail differences in the teachers' practice. A third element of the study involved interviews with the 32 classroom study teachers. The interviews provided a vital link between the rhetoric of the questionnaires and the reality of the classroom practice observed. They were crucial in making the explanatory links between perception and practice and provided not only the explanations needed to understand the relationship of one to the other but also in triangulating the veracity of the other data sources.

The other studies to which I will refer are the ESRC-funded 'Primary Assessment Curriculum and Experience (PACE)' (1989–1997) and the 'Systems. Teachers and Educational Policy (STEP)' (1994–1996) projects. The former is an English study which was primarily designed to study the impact of the National Curriculum and its Assessment on English primary schools. However, part of its conception was also to build on the earlier findings of the Bristaix project in order to map how far English primary teachers' professional priorities might have changed in the years between 1985 and 1997. The STEP project was conceived as the fourth quarter of the comparative and historical matrix of studies which is depicted in Fig. 1. It was designed to study French primary teachers' responses to similarly radical and imposed policy reforms eight years on from the Bristaix study.

As a group, these projects provide a relatively rare example of studies which have been designed to be 'cross-cultural' in three different ways: internationally, intra-nationally and historically in the broad field of comparative education. A cursory scrutiny of the leading comparative journals, such as 'Comparative Education' and 'Compare' (edited in the U.K.) and 'Comparative Education Review' (edited in the U.S.) reveals a field typically dominated by one country studies or more comparative, largely policy-oriented accounts.

France	1984–1987 Bristaix	1994–1996 STEP
England	Bristaix	PACE (1989–1997)

Fig. 1.

There remains a dearth of theoretically-informed and explicitly designed qualitative comparative studies which take as their empirical focus either practice or participants' subjective perspectives.

More significant still is the frequent equation of *cross-cultural* with *cross-national* and the particular dearth of *intra*-national comparisons between different cultures, regions and socio-economic locations. As Fig. 2 shows, this design provides an almost unique range of cross-cultural comparative possibilities between for example – rural (and urban) French primary teachers before and after recent reforms, or between rural and urban French primary teachers in the same period or indeed, a 3-way comparison between rural French and English primary teachers before and after their respective major reforms. In both the Bristaix projects and the subsequent PACE and STEP projects, provision was also made in the research design for *intra*national comparison between teachers working in schools in different socio-economic environments – rural, inner-city, suburban and affluent – the intention was to use different kinds of cultural comparison to illuminate key sources of influence.
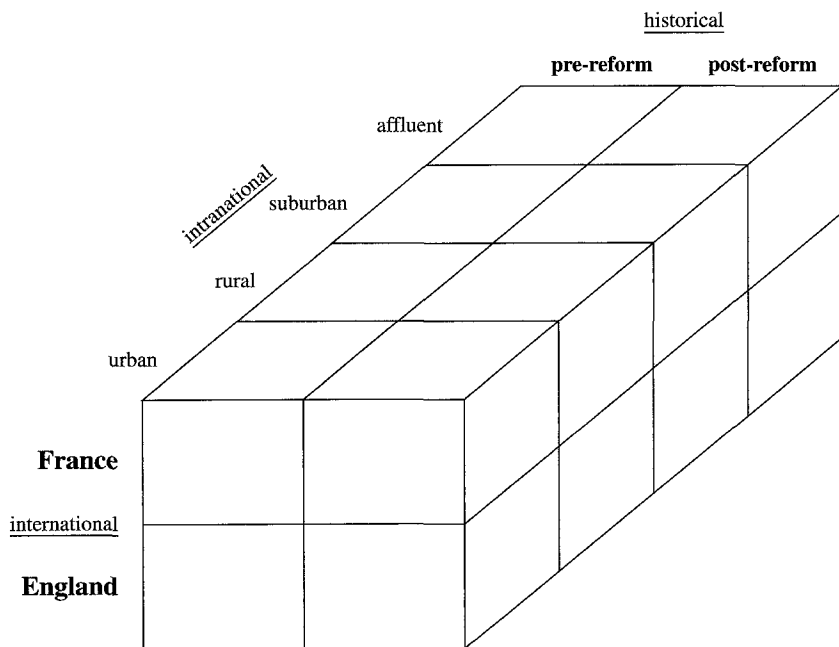
Fig. 2.

Collecting Interview Data in a Cross-Cultural Context

The problem of achieving matched/equivalent samples of respondents is thrown into sharper focus when cultural factors may determine differential possibilities of access. In France, for example, in the Bristaix study, the lack of an established tradition of qualitative research in general and of classroom research in particular meant that there was widespread resistance on the part of schools and LEA personnel to the provision of access to teachers in order to interview them. Indeed, without a network of personal contacts, achieving access to schools and teachers would not have been possible. Even having achieved access, significant practical problems to the creation of an effective interview situation were posed by the lack of common room facilities, such as a staff room, in many schools, as did working practices which meant that teachers were typically at home when not actually teaching. This meant imposing on their free time to interview them.

The problems of access and the willingness or otherwise of key personnel to facilitate the collection of interview data were also experienced in the subsequent comparative studies, PACE and STEP. The PACE project was funded in, 1989 and though it incorporated questions from the BRISTAIX interview study it inevitably had to focus on topics which were to the fore of educational discussions at the time that it was funded. The STEP project was funded in, 1993 and though it was explicitly designed to build on BRISTAIX and PACE. it was also necessary that it should reflect the predominant concerns of that particular time.

In a sense the three related projects provide a particularly pertinent illustration of the problems of data collection in contexts which are the product of different systemic cultures; of different languages and also, of the educational policy cultures of different times. An extended process of translation, refinement and re-translation was necessary to ensure that the questions asked were meaningful to respondents. This resulted in a significant number of 'nuances' becoming incorporated into the questions such that the core of comparable questions and hence data were steadily reduced over time. Thus there are considerable practical difficulties associated with designing instruments which have the same salience and cultural resonances for each population. It is often difftcult to frame questions which are intended to be comparative in that they address the same issue in two different cultures which are at the same time equally meaningful in both those cultures.

Such issues of research validity may be illustrated by the following examples. In interviewing French and English primary teachers it quickly became evident that an apparently neutral notion such as 'teaching style' had

a completely different resonance in England than it did in France, where teachers found it difficult to conceive of their practice in terms of a 'teaching style'. For them teaching was largely unexplicated and unproblematic. By the same token, another cultural concept which was central to the study – 'accountability' – has considerable resonance in the English language, yet is largely inexpressible in the same terms in the French language.

There remains also the obvious difficulty of obtaining linguistic equivalence through translation. For a time 'back translation' was seen as the answer to these difficulties. In this procedure, research materials are translated from language A to language B by a native speaker of B, then from B to A by a native speaker of A, then from A to B by a third party and so on until discrepancies in meaning are clarified or removed as much as possible. However, this approach has been criticised since it can convey a spurious lexical equivalence. As Deutscher (1973) has suggested, "since language is a cultural artefact it must be assumed that the question is being addressed to people who are immersed in two different cultural milieu." To the extent that this is so, it is not aufficient to know simply that the words are equivalent, it is necessary to know the extent to which those literally equivalent words and phrases convey equivalent meanings in the two languages or cultures.

It is this need for the two different cultural perspectives to be represented at the stage of both instrument-design and data-collection which reinforces the importance of having an international research team. Hantrais & Mangan (1996) refer to the 'safari' approach to cross-national research in which a single nation team of researchers carry out studies in more than one country. Whilst this has the benefit of consistency of perspective, reseach design and intellectual culture, it is also very vulnerable to cultural bias. The 'Bristaix' project, was conducted by an international team from England and France. The interview stage of the project, however, was conducted by a 'safari' team. Two experienced English teachers of French were appointed to carry out the interviews because of the high level of linguistic competence required. Both were given extensive training in the use of the schedule which was piloted in England and both had considerable practice in using it. Nevertheless, there were differences between the styles of the two interviewers in the way they translated and transcribed their interviews and especially in their cultural interpretation, despite a sustained effort to combat this by training at the outset. The same problems were subsequently manifest in the STEP study in which, again, without active French collaborators, the need for fluent French speakers to conduct interviews resulted in the project having to employ some staff specifically for this purpose.

Analysing Interview Data in a Cross-Cultural Context

We turn now to the problems of analysing interview data in a cross cultural context. In an early ESRC-funded comparative project based on interviews – 'Constants and Contexts in Educational Accountability' – which was designed to uncover the micro politics of policymaking in two different education systems, the interviews were deliberately very unstructured and open-ended. The result was that respondents tended to talk at length on matters of their own choosing with relatively little prompting by the interviewer. The effect of this, as with all such open-ended interviewing was to produce unanticipated and valuable research insights. The disadvantage was, predictably, the resultant difficulty in comparing the responses of interviewees even within the same cultural settings, let alone across cultures. Nevertheless there is a strong argument for the creation of such idiosyncratic accounts since the subject matter under study, in this case policy making within the education system, was so different in the two cultures. By contrast, the more recent studies of teachers' changing perspectives about their work referred to above have been based on rigorous attempts at matched interview schedules which combinine a mixture of closed and open-ended questions. An example of these is given in Appendix 1.

In seeking to analyse such data a number of problems present themselves. As with data collection, the overriding problem is the question of language. Interviews must be conducted by interviewers who are fluent in the language in question. However whether they are operating in their own education system or in one that is not their own will determine the presence or absence of a whole set of subtle nuances of interpretation on the part of the interviewer. A decision had to be made concerning for example, in each of the projects referred to here, whether to use *native* interviewers in both England and France or to use the *same* interviewer in both France and England. This decsion is likely to be a significant one either way, not only from the point of view of what the interviewer understands, but also from the point of view of what the interviewee thinks it is appropriate to say. In other words, French teachers are likely to perceive an English researcher in a different light from a French one. In our most recent studies we have come as close as possible to minimising this effect by using biligual, bi-cultural researchers.

A more specific language problem concerns the recording of the data. Where tape recording is used, notes are likely to be written in a mixture of English and French. Then a decision has to be made as to whether, and when, translation into the working language is to be made. The best solution to this problem may well be to use a bi-cultural researcher to amplify the interviewer's written notes

using the original tape recordings in order to make a new set of notes in English whilst keeping quotations in French.

The extracts below however, illustrate the idiosyncratic tone of many of the responses generated and the consequent difficulty of capturing the very different world views which underpin the utterances of teachers working in different cultures.

A votre avis pourquoi la réforme Jospin n'a-t-elle pas eu des'effets plus importants sur l'enseignement?

Jokes – don't know the Jospin laws that well!
Says: Evidence that everyone knows it, but no one does it. The most experienced teachers don't want to move or to change the way they work – would have to do a lot of supplementary work!

Ministers come and go – all seem to need to make a different reform. Then a year later they change and everything is rescinded. To do things differently, you need training. It hasn't been provided. Policy makers work in an office "Ils sont très loin du terrain." Their ideas are often inapplicable.

They don't always give the means to do it properly "Liberté de l'enseignant" is always used as an argument for not changing.

Je dis liberté dans la manière de l'enseigner 'oui' mais pas liberté dans les programmes, les' objectifs et les resultats!

Always teachers who 'ne sont pas d'accords' – some don't adapt well to working in a team – keep themselves shut in classroom.

With them, hard to get them to share ideas. Have always worked in groups and still do, but would like to have more resources to do it.

"Il ne m'appartient pas de juger la Réforme Jospin. Certes, sur le plan des 'techniques', elle a amené des modifications: organisation en travail de groupe, d'équipe, des concertations plus soutenues, des 'réunions' de cycle, des évaluations-continues, l'élaboration de nouveaux livrets, une écoute de l'enfant plus aiguë. Dans notre établissement, l'expérience du 'décloisonnement' a permis de libérer des heures de soutien . . . Bien sûr, il est encore trop tôt pour apprécier les progrès de nos élèves en difficulté, ce que demeure notre soucis premier. Mes élans pour cetter nouvelle conception vent bienveillants mais seront modérés.

(French teacher's response in French)

It's not for me to judge the Jospin Reform. Certainly, in terms of teaching methods, it has brought changes – the organisation of our work in groups, in a team, more regular consultations, meetings for each 'key stage', continuous evaluations, the development of new record books, a more careful focussing on the child. In our school, the experience of 'unlocking' has allowed hours of support time to be freed up. Of course it is still too early to judge the progress of children with learning difficulties – something which remains our first concern. My hopes for this new approach are positive but modest.

Finally we turn to the problem of writing up and reporting such data.

Reporting Cross Cultural Interview Data

From the foregoing discussion, it will be apparent that it is very easy to misinterpret data which were generated in two different cultural contexts. It is easy to ask the wrong questions, to focus on issues which are important in one national context but not in another, and to misunderstand the deeper assumptions on which the responses are based. The problems of validity which beset attempts at systematic comparison are less likely to be a problem for more ethnographic studies. In the latter approach where the emphasis is on describing the whole in its own terms, it is much easier to protect the integrity of the perspectives and responses of the participants being studied. Comparative *case studies* have considerable insights to offer in terms of 'making the familiar strange' and this is no doubt why so many comparativists avoid the kind of systematic attempts at comparisons using a standardised research instrument of the kind described here. Interviews can clearly provide for both kinds of approach and in the choice of methodology within this broad category it is important to be aware of the limitations of each in a cross-cultural setting.

Equally, there are nuances of culturally-influenced interpretations which will affect the way in which findings are reported. Certainly in France and England, the rather different intellectual traditions in which educational research is embedded, as well as the different questions being asked of the research makes it necessary to report findings in varying styles. Thus the insights distilled from interviews will be further mediated in the reporting process by the culturally selective filter of what is felt to be important and appropriate to report. In the BRISTAIX project, for example, the book which was the final outcome of the project – *Perceptions of Teaching – primary school teachers in England and France* was not only published in two different languages by two different publishers. The text had to be altered quite considerably to meet the perceived criteria of each national team concerning quality and relevance.

OVERVIEW

As we move into an era in which collaboration in educational research between the partner countries of the European Union in particular is likely to become increasingly common, it is vital that we develop new understandings about how the inevitable influence of culture and tradition affects not only what is *done* in the name of education but equally, how this can be *studied*. Such understanding needs to include a positive awareness of the great potential value of such inter-cultural studies in their ability to challenge deeply-rooted ethnocentric

assumptions. Not only are they a rich and ready-made laboratory for the educational researcher, they are virtually the only means available of iterating key variables in the study of educational systems. But this powerful tool is also fraught with the pitfalls discussed in this chapter. It is not for the unwary or for those unwilling to struggle with a very different kind of semantic differential test. Interviewing in a cross-cultural context provides one of the greatest challenges for contemporary social scientists. The inevitably delicate process of constructing interaction and communicating meaning which interviewing represents, in which many different cognitive and affective facets must be managed if the encounter is to be successful, is rendered infinitely more vulnerable when the common currency of language and cultural assumptions is removed. As such, interviewing in a cross-cultural context may legitimately be regarded as the ultimate phenomenological challenge!

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APPENDIX 1

Teacher Interview Schedule: STEP Project

England

Do you ever work with other teachers? For example do you work with teachers who have a parallel class, or with teachers in the same 'cycle' as you?
Could you describe how this is organised?

Does the transfer from CE1 and the 'Cycle D'Apprentissage Fondamentaux to CE2' and the next 'Cycle D'Approfondissement' raise any issues for your work as a teacher?

We have been talking about your classroom practice and your role as a teacher. Overall, do you think that your role as a teacher has changed much in the last few years?
If so, can you say how.

Cover

Freedom/constraint

Working with colleagues

Enjoyment of teaching

Personal fulfilment

Do you find as much satisfaction/fulfilment in teaching now as you did five years ago?

(If teacher has changed) Who, or what, has provided the most support or influence in helping you to adapt to new demands?

What changes, if any, do you still plan to make?

Looking back over the last five years, can you say if a typical working day has changed in any way

France

Vous arrive-t-il de travaille avec vos collègues? Par exemple travaillez-vous avec un instituteur/institutrice d'une classe parallèle, ou avec des instituteurs du même cycle?
Pouvez-vous me dire comment vous vous organisez?

Est-ce que le passage du Cycle d'Apprentissage Fondamentaux du CE1 au Cycle d'Approfondissement du CE2 vous inspire des réflexions quant à votre travail d'enseignant?

Nous venons de parler de votre rôle d'instituteur et de votre pratique dans la classe. D'une façon générale pensez-vous que votre rôle d'instituteur a beaucoup évolué ces dernières années?

(If yes) Pouvez-vous indiquer de quelle façon?

Cover

— Liberté/contrainte

— Travail avec collègues

— Le plaisir d'enseigner

— Epanouissement personnelle

Trouvez-vous autant de satisfaction à enseigner aujourd'hui qu'il y a quelques années?

(If teacher has changed) Qu'est ce qui ou qui vous a le plus aidé à vous engager dans la direction de cette réforme?

Quels changements, s'il y en a, aimeriez-vous encore faire?

En considerant les cinq dernières années pouvez-vous dire si votre journée scolaire a changé d'une manière ou d'une autre?

DECONSTRUCTING THE QUESTION: REFLECTIONS ON DEVELOPING A COMPARATIVE METHODOLOGY FOR RESEARCH ON UNION POLICY TOWARDS VOCATIONAL TRAINING

Estelle Krzeslo, Helen Rainbird and Catherine Vincent

Editors' Commentary: *Research into trade's union policies towards vocational training in five countries provides the context for this chapter. Kreslo, Rainbird and Vincent explore cross-cultural case study in relation to language and meaning. They stress the significance of contextualized meaning in relation not only to what they describe as national realities, but also to the way in which the actors perceive such realities located in different countries. The chapter outlines a technique of 'crossed interviewing' whereby researchers of different nationality attend the same interviews, in an attempt to avoid the ethnocentrism of the cultural specificity of the lone interviewer. The approach facilitates a common methodology but at the same time, allows flexibility towards different national realities. The authors also discuss the shortage of research funding available for cross-cultural research. They argue that a narrow concentration on national experts with specialist knowledge is not conducive to comparative methodology.*

INTRODUCTION

Vocational training is a topic which has inspired many comparative studies. The reason for this is not hard to find: vocational training is perceived as contributing to economic competitiveness and so both the stock of skills in national workforces and the institutions which contribute to the development of the skills base have been the subject of study. National governments and research institutions have funded research in this field as have European institutions, both the European Commission and the European Centre for the Development of Vocational Training, CEDEFOP. European Union policy on vocational training and, in particular, concerns with the comparability of vocational qualifications, the fight against social exclusion and the need to create policy instruments which will promote the updating of the skills of workers in employment, have been informed and accompanied by comparative European research in this field. Therefore forces both external and internal to academic research concerns have contributed to the development of comparative research in this field (cf Oyen, 1990). In each European country, vocational training intersects with the institutions of industrial relations. In this academic field there are also powerful forces promoting comparative study. On the one hand, this has been prompted by the internationalisation of economic activity and, in the European Union, the development of supranational policy and a regulatory framework which needs to take account of distinctive national institutional systems. These two areas come together in research which analyses the roles of the two parties to the employment relationship – the employer, on the one hand, and unions, on the other – in relation to the development of policy and practice on vocational training. This is particularly significant at the present time since vocational training is an important constituent of the process of social dialogue at European level, as well as contributing to the management of change within companies.

   This chapter starts from the premise that this policy context can not be dissociated from questions of research methodology. The existence of European funding for research on vocational training, for example through the Leonardo programme, means that policy concerns with the relationship between training and competitiveness and with training as a tool of labour market policy exert considerable influence on the research agenda. The objective of these research programmes is to develop European Union policy in this field and not to generate abstract scientific knowledge. It means that policy objectives define the way in which the research agenda is formulated and how research questions are posed. We argue that, far from being unproblematic, the definition of research questions in terms of a particular policy agenda requires

researchers to reflect on their terms of reference and to examine the meaning of concepts derived from the policy debate in each national context. This requires a process of deconstruction of the research question and its problematisation. We will illustrate this in relation to the French policy concern with *les bas niveaux de qualifications* eg (workers with a low level of qualification) which was the starting point for our own comparative research.

Funding regimes for comparative research impact on research methodology in other ways. They rarely allow for teams to conduct research in different countries, but promote the development of networks of relationships between 'national experts'. This allows specialist knowledge of each national context to be drawn upon and appears to be a cost effective method of conducting research in several countries at the same time. We believe that the use of specialist knowledge is not necessarily conducive to the development of a comparative methodology. Unless there is a process of exploring different national realities and the ways of conceptualising them, there is a danger that national studies are conducted which are simply juxtaposod to each other. Whilst *juxtaposition* may go beyond a simple *description*, of national institutional systems to explore what these frameworks mean for practice, we believe that a *comparative methodology* requires researchers to distance themselves from their own national reality and to question the nature of both institutions and practice.

Having outlined some of the difficulties in conducting multi-country comparative research in the field of vocational training, we will discuss our experience of working on a project on union policies towards the training of unskilled workers in five countries. In the first section, the context and background to the project is examined, including a discussion of the role of language in fieldwork and, in particular the role of an innovative method, the 'crossed interview' (cf Dufour & Hege, 1993) in conducting comparative research. We then outline the process of negotiation of research design, emphasising questions of autonomy and comparability. This is followed by a discussion of the key findings of the research and how a comparative approach resulted in the raising of new research questions.

Background and Context of the Project 'Union Policies Towards The Training Of Workers With A Low Level Of Qualification'

Unemployment is one of the major challenges confronting the member states of the European Union. The problem of social exclusion has been identified by the European Commission as a significant policy concern. Unemployment is one of the most important forms of social exclusion insofar as it contributes to

economic disadvantage and to the overall weakening of the social relations of
the unemployed and the social cohesion of communities. Despite the
significance of unemployment in European social and industrial policy,
research on unemployment tends to focus on individual career trajectories and
the process of reinsertion, on the one hand, and the affect of policy measures
designed to combat closures, on the other. Research in industrial relations has
rarely focussed on the representation of populations who are vulnerable to
unemployment and the processes which result in their marginalisation. In this
research project, we attempted to consider these questions with respect to the
role taken by trade unions as actors in combatting the exclusion of workers who
are least skilled through their policies towards training.[1]

The skill level of workers with a low level of qualification has only become
a problem in the context of economic crisis and the technological changes
which have produced a massive reduction in employment and in the demand
for unskilled labour in particular. For, as Alaluf and Krzeslo (1993) argue, so
long as they were employed, the low level of their qualifications was not
problematic. Paradoxically, the question of workers with a low level of
qualification has arisen at a time when, overall, levels of workforce
qualification have risen. In this context, in France, continuing vocational
training has been perceived as one of the principal policy instruments for
fighting unemployment, with the objective of reducing the supposed gap
between the supply of labour and employers' demands. The training of workers
in employment has been perceived as the mechanism for adapting the labour
force to the requirements of new forms of production. There are therefore two
dimensions present in the question of training: the insertion of the unemployed
and the retraining and reskilling of employees. The logic of this policy does not
address the question of the reduction of employment demand which all
European economies have experienced.

For union organisations, training is not a straightforward issue as it has
different implications where it concerns those who are in employment in the
company and those who are outside it and have few possibilities for reentering.
With few exceptions, training has generally been a low order priority for unions
and questions concerning retraining in relation to new forms of production have
been identified in relation to the most highy skilled employees rather than the
least skilled. This is reflected amongst researchers, who have debated the
consequences of new forms of production for the demand for higher level
skills, alongside an increasing polarisation in the labour market between more
highly skilled workers and those in peripheral or 'non-standard' jobs.

The extent to which the retraining of these workers has been identified as a
significant public policy issue and the way in which it is articulated varies

between countries. In the same way, the extent to which unions are able to influence policy outcomes is constrained by the configuration of the system of industrial relations, and the existence of institutions designed to involve unions in decisions on vocational training and redeployment. Insofar as unskilled workers are often poorly represented in union organisations, a focus on this category seemed particularly fruitful. It allowed the project to examine the effectiveness of union strategy with respect to reinsertion and retraining, but also the difficulties of representing and integrating the interests of different groups of workers and, in particular, those whose structural position in the labour market is weak.

Methodological Issues Raised by the Research

Despite the development of comparative study both of vocational education and training (VET) systems and industrial relations, these fields are fraught with problems of comparability. This is partly because VET institutions are the product of distinctive national histories and interact in different ways with national systems of industrial relations, employment regulation, patterns of work organisation and job descriptions. To give two examples, the concept of 'skill' in Great Britain is rooted in the tradition of apprenticeship and the organisation of craft workers who combined to restrict the availability of skilled labour. Although *'qualification'* in French is the functional equivalent of this term, it is rooted in the education system and in promotion through seniority in the workplace. Because the terms refer to the characteristics of individual workers and structural features of the organisation of production and are found at the intersection of the systems of education and production, they are widely debated (see, for example, Tanguy, 1986). In the British literature, the concept of skill is often poorly defined and conflated with questions of craft job controls and the exercise of work autonomy (Rainbird, 1997). Feminist writers such as Cockburn (1983) and Philips & Taylor (1986) have argued that skilled work has been socially constructed as male work. In a similar way, even though a term such as 'apprenticeship' can readily be found in many countries, its status, the range of occupations it covers, and its ability to lead on to further education and promotion vary widely (Campinos-Dubernet & Grando, 1988).

There are further problems concerning the way in which social scientists from different intellectual traditions communicate with each other, especially in an interdisciplinary field which may be significantly influenced by national policy debates.[2] Researchers must be able not only to understand different national realities but to understand how these realities are perceived by the actors themselves and their own colleagues. If these issues must be confronted

by an individual researcher conducting a detailed comparative study of two
countries, this is more complex where multi-country comparative studies are
concerned, involving a number of different national teams.

Many questions concerning the methodology and interpretation of compar-
ative research have been raised through the study of other cultures and
societies, for example, in British social anthropology and French ethnology.
For Malinowski, the 'father' of the British ethnographic fieldwork tradition,
learning the language and inserting oneself in a culture were essential in order
to "grasp the native's point of view" (1922). The dangers of relying on
missionary accounts, the information of key informants and other commenta-
tors are well known. This is due to the fact that they may have an interest in
presenting a particular point of view or in using their position to pursue
personal objectives. These observations are as relevant today where social
scientists are being encouraged to engage in research on other countries in a
context of the completion of the Single European Market and the transforma-
tion of the former Eastern Bloc states. An ability to speak and read the language
of the country being studied is central to the comprehension of the categories
through which individuals apprehend their own social reality and institutions.

In an ideal world, researchers would have the time and the resources to
conduct comparative research themselves. However, particularly in the
European context, once the study of more than two countries is envisaged, it
becomes increasingly difficult to acquire the linguistic skills to conduct this
comparative work personally. This is one reason why comparative studies have
involved the setting up of teams based in different countries (for example, the
organisation of business interests project, see Streeck & Schmitter, (Eds),
1986). This is a practical way of conducting comparative research whereby
expertise of national specificities can be integrated into the project. The
participation of researchers from other countries can be extremely fruitful in
the extent to which each team brings its own conception of what their colleages
are analysing, but also tries to understand their perception of it. Developing a
comparative methodology involves challenging ethnocentric assumptions and
negotiating a research agenda which can accomodate a range of different
conceptions of the research focus and methodology.

Linguistic competence contributes to the communication between members
of the different teams and to the understanding of the differences between
institutions in each country. However, in the field of industrial relations, it has
been recognised that the translation of terms can be misleading if they are not
placed in the context of the legislative and collective bargaining traditions of
each country. Industrial relations glossaries which explain terms in their

context rather than translate them have been published in order to facilitate understanding of different legislative and collective bargaining systems.[3]

The Negotiation of Research Focus and Method

As we have already outlined, the point of departure of our research project was derived from the French policy concern with the employability of workers with a low level of qualification – *les bas niveaux de qualifications,* BNQ. It soon became apparent that this was not a statistical category which could be easily identified in all the countries. The first question which was posed was whether the population concerned was one which was defined by its low level of formal educational qualifications and/or vocational training or with respect to its structural position in the organisation of production. This question revealed the polysemic nature of the concepts of 'skill' in English and *'qualification'* in French, which refer not just to individual characteristics and the job occupied but also to the classification of jobs and workers in a hierarchical manner, which is the outcome of negotiations between management and labour. These concepts are therefore intimately linked to different trade union traditions and the organisation of work in each country.

Despite well-documented differences in levels of workforce qualification between countries, the category of unskilled workers/workers with a low level of qualification could be identified in all countries. It is perhaps useful to point out at this stage that one of the first tasks of the project was to define what was meant by 'workers with a low level of qualification'. It was decided to focus on those whose structural position in the organisation of production resulted in them being classified as unskilled, rather than the formal qualifications of individual workers. This was for two reasons. Firstly, a focus on qualification as a characteristic of individual workers would have created problems of equivalence between countries which have relatively high levels of workforce qualification and those that have much lower ones (the differences between Germany and Britain, for example). Secondly, although in some countries, formal qualifications are recognised in job classification hierarchies and salary structures and, in this respect, an equivalence can be perceived between qualifications and structural position, this is not the case everywhere. Therefore, for the purposes of the research, workers with a low level of qualification were defined as being unskilled workers, because the objective was to examine structural disadvantage in the labour market. It was agreed that the research project should focus on the strategies of unions with respect to groups of workers who are disadvantaged in the labour market, whether they are called *'bas niveaux de qualifications'*, as in France, *'groupes à risques'*, as

in Belgium or 'peripheral workers' as in the U.K. However, this raised a further problem insofar as some union organisations can be categorised as defending only the sectional interests of their members (and the unemployed may be included in or excluded from this category), whereas others perceive themselves to represent the interests of broader sections of the working class, enshrined in ideologies of social and political solidarity, and this may also be recognised in labour law.

From the first meetings of the teams, the establishment of a common grid of questions appeared to be impossible. The teams wanted to go beyond a description of the different legal and institutional mechanisms and the place of unions within these structures. This was for two reasons: on the one hand, these descriptions were already available, for example through the publications of CEDEFOP. On the other hand, the objective was to go beyond an understanding of institutions as normative and ideal systems and to study the practices of the actors. We therefore decided to allow each team to interpret the subject with regard to national specificities and then to reflect collectively on the different meanings which were hidden behind these categories which appeared to be similar.

In the same way, in the discussion of the development of fieldwork methodology, the teams had to confront the question of the levels at which interviews with union representatives should be conducted. The concept of a union covers many different realities in terms of priniciples of organisation, degrees of centralisation, the level, depth and extension of negotiation etc. which made it impossible to take a prescriptive approach to identifying levels at which interviews should be conducted. As a result, although agreement was reached that the focus should be on workplace issues in three sectors (motor manufacture, food processing and health care), a high degree of autonomy was delegated to the teams in identifying the higher levels of negotiation which were significant in affecting practices in the workplace.

The solutions adopted generated tensions between the autonomy delegated to national teams and the need to develop a common interview schedule. To a certain extent these tensions were resolved by the use of an innovative research method, the crossed interview technique, which was developed by Dufour & Hege in their comparison of levels of representation of employees in France and Germany (Dufour & Hege, 1993). In each country members of different national teams attended the interviews. So, for example, Belgian and German researchers participated in the interviews in Italy; French and British researchers in Belgium; French and Italian researchers in Great Britain etc. Even if not all the teams were represented in all the interviews, these exchanges allowed a degree of homogeneity to be established in the interview schedules.

More importantly, it allowed the team members from different countries to observe and experience the operational realities of the different union organisations in a way which was often illuminative. For example, the complexities of the linguistic and catholic/socialist divide in Belgian industrial relations are often difficult for a foreign observor to grasp, without direct experience of it. Whilst the claim of *all* the French trade union confederations to represent the interests of the whole of the working class despite low levels of unionisation stands in stark contrast to the sectional organisation of British trade unions, despite much higher levels of union membership. In the same way, the presence of 'outsiders' in the interviews, whose experience derived from different industrial relations systems, raised new questions for research. For example, in the British interviews the Italian and French researchers were interested to investigate how wage classification systems were established, given that there are no sectoral level classificatory systems. The interviews in Belgium in the food and drink sector established that the capacity of unions to act on behalf of unskilled workers in the brewing industry with respect to retraining and redundancy were remarkably similar to those in Britain despite widely differing frameworks of industrial relations and employment law.

  This experience of comparative research demonstrates that teams need to be prepared to negotiate a common methodology whilst maintaining a flexible approach towards the different national realities. In order to do this, they must be able to recognise the validity of the concerns of research questions identified by each participant and to accept the possibility of finding innovative solutions. The crossed interview technique contributed to mutual understanding between the members of the research teams and created a dynamic which allowed the hypotheses of the research to be questioned.

Research findings

From the, 1980s, unions have been confronted by increasing and persistent unemployment and the problems posed by the adaption of the labour force to organisational and technological change. Three factors have contributed to their ability to react and take the initiative in this context. Firstly, the balance of power has been unfavourable to union organisation and this is evident in the decline in union membership in most European countries. Secondly, there has been a process of decentralisation in collective bargaining to the company level. Increasing demands by employers for efficiency and competitiveness have resulted in the shift in emphasis in negotiations from higher level structures, giving companies increased flexibility and the ability to exercise management prerogative, especially where unions are weakly implanted at this

level. Thirdly, training plays a privileged role in management strategy in developing new forms of work organisation. These initiatives include multi-skilling, as well as developments such as 'management delayering' which require some devolution of decision-making to shopfloor workers, especially with respect to quality assurance and maintenance functions. In the latter context, there is some scope for unions to engage with management in the discourse of 'Human Resource Management' and 'empowerment'.

One of the significant findings of the research is the difficulty encountered by union organisations in influencing questions of retraining and reskilling at workplace level. Even in a country like Germany where the system of initial training is strongly structured and regulated by the social partners, continuing training is managed and controlled by employers. For a long time, unions and works council representatives have been content to take a passive role in this domain, leaving the initiative and implementation of training policy to management. However, our interviews demonstrate the desire expressed by union leaderships to develop a new strategy with respect to training and reskilling. This development is not found in all the countries surveyed, but particularly in Italy and Great Britain. For different reasons, union organisa-tions see demands for vocational training (especially for the least skilled workers) and for representation on matters concerning it as a means of seeking a more consensual form of industrial relations (cf Rainbird & Vincent, 1995).

Although there has been considerable discussion of 'a new bargaining agenda' for trade unions, our study demonstrates that they have not succeeded in extending this agenda beyond traditional quantitative concerns with wages and conditions. The training and retraining of employees continues to be subject to management prerogative and it is the employers who have been taking initiatives in this field. In contrast, it is generally the state which has taken responsibility for the retraining of the unemployed.

In all the countries in the study it is significant to note that regardless of the existence of legal obligations or collective agreements at sectoral level, and despite the existence of budgets earmarked for training, without exception, the least qualified workers are those who have least access to further training. This is the case, even in France, where training is regulated by the law, where there are employee rights to consultation and information on training and, moreover, an individual right to training leave (Rucart, 1993). In Britain, it has often been argued that a deregulated training system contributes to the reproduction of labour market disadvantage (Equal Opportunities Commission, 1990). It would appear that whilst regulation may contribute to the *volume* of training, it does not challenge its *distribution.* This raises fundamental questions about the way in which trade union action contributes to the maintenance and reproduction of

workplace hierarchies. It is at this point that comparative study began to raise new questions for research.

From the fieldwork findings, it was clear to all the participants that in Great Britain, trade union organisation and the structures of collective bargaining create barriers to access to training and to internal mobility. Traditionally, amongst manual workers in manufacturing industry, only those entering skilled occupations organised by craft unions have had access to training through the apprenticeship system. The existence of age limits, alongside the operation of apprentice ratios and job controls have had the effect of excluding manual workers from access to training. British employers colluded in these practices and, themselves, had little interest in increasing the numbers of skilled workers they employed. With the development of new technology, some skilled workers have been reclassified as technicians. The existence of multi-unionism and multiple bargaining units at plant level has meant that occupational mobility resulted in individual workers having to join the unions which held negotiating rights for their new occupations. Indeed, many of the failed and successful union mergers of the last twenty years have been initiated between unions which were in competition for members, competition which has been exacerbated by changing technology and increased flexibility in working practices (Rainbird, 1989). Even in large organisations operating internal labour markets, such as the banks, internal mobility through access to training has been rare for workers on the lowest grades. Indeed, for many female workers, leaving a job and seeking retraining outside the workplace, has been the only means of obtaining job mobility (Payne, 1991). Therefore in a real sense, not only have there been few opportunities for manual workers to access training and job mobility, but the practices of trade unions and, in particular, those of craft unions, have contributed to limiting the access of the least skilled workers to training. Furthermore, the formal exclusion of the Trades Union Congress from the field of public policymaking has restricted its ability to act authoritatively with respect to active labour market policies.

A superficial analysis of the situation in the other countries would suggest that their union movements do not represent such narrowly sectional interests in the same way. In Belgium, unemployed workers continue to be union members and, in fact, unemployment benefit is administered by the unions, which explains the high levels of unionisation here. It is the only country in our sample in which there have been national collective agreements on the allocation of funds from the wages bill to training for 'groups at risk' and reflects this broader membership base and the solidaristic ideologies of Christian Democracy and Socialism. The quota was fixed at 0.18% of the wages bill for 1989–90 and at 0.25% in the agreements for the periods 1990–2

and 1992–4 (Alaluf & Krzeslo, 1993). In Italy, the major union confederations also have solidaristic political ideologies, and are recognised in labour law as having 'representative status' (Ferner & Hyman, 1992). In Germany, the institutions of collective bargaining at sectoral level and of worker representation (through the works councils) at company level make decisions on behalf of the entire workforce. The claim of German unions to represent all employees and not just their members is enshrined in law (Jacobi et al., 1992). In France, despite low levels of unionisation, the state has granted representative rights to the major confederations, which allows them to represent all employees and not just their members. Social benefits are provided through legislation, rather than through collective bargaining (Goetschy & Rozenblatt, 1992).

However, in raising questions about the mechanisms reproducing workplace hierarchies, the project demonstrated the strengths of examining practice, as opposed to institutional systems. Despite the solidaristic nature of Belgian industrial relations and the agreement concerning 'groups at risk', the fieldwork interviews found that shopfloor workers at Volkswagen were resistant to the allocation of training places to the unemployed. This was because informal recruitment practices operated whereby family members of employees received preferential treatment. If the interests of the unemployed were prioritised, then this had a detrimental effect on the job chances of workers' children. In Germany, the unions were interested in using policies towards vocational training to attract more highly skilled workers rather than to appeal to the least skilled. It is these groups of workers who are perceived as having greater bargaining power and who might be attracted to unions which are active in their strategies towards training. Particularly in motor manufacturing, there are many skilled workers, usually trained in the handicraft sector, who are employed on assembly lines as unskilled workers. Therefore, here the problem is one of overqualification for the jobs available rather than a problem inserted into the catering sector, where wages and conditions were inferior (Rehfeldt, 1993).

It is only in Britain that union recruitment is explicitly linked to membership of different occupational groups. Nevertheless, it is often the case in other countries that occupational categories within the same workplace are organised by union, confederations which are distinguished by their political ideology. This was the case in Renault, where the union confederations took up very different positions with respect to the 'accord à vivre' of 1989, which concerned the forward planning of employment and work organisation. The Confédération Générale du Travail refused to sign the agreement with the company whilst the other trade union confederations accepted that there would be redundancies and redeployment of staff but perceived that it was important

to exercise some influence over the decision-making process (Vincent, 1993). Moreover, even where different categories of worker are organised by the same union, there may be a conflict of interest between categories. In France, in the health care sector where qualified auxiliary workers have resisted the regrading of workers doing the same job who have learnt their skills through experience (Rainbird & Vincent, 1995). Although the division of unskilled and skilled workers into membership of different unions, as in Britain, would appear to hold a number of disadvantages for union organisation, there are also some benefits from having unions which are not dominated by the interests of more highly skilled workers.

The bringing together of policy and academic debates from different countries can also enrich comparative analysis. Union discourse on equality of opportunity is well developed in Great Britain and can be attributed to the existence of Equal Pay legislation since 1970. Its failure to address pay inequalities have given rise to concepts of 'comparable worth' and a critical appraisal of techniques such as job evaluation which are supposed to measure in an objective way, the complexity, technical skill and responsibility attached to particular jobs. This, combined with feminist analysis of the social, political and gender construction of notions of skill (for example, Cockburn, 1983) has contributed to a questioning of common sense understandings of skill as expressed in job hierarchies and, in particular, the systematic underevaluation of women's skills. These issues are not debated in the same way elsewhere and where sectoral level job classifications are negotiated, as in France and Italy, they may appear to be objective. However, job segregation and labour market segmentation are found in all the countries. This would seem to raise important questions about the objectivity of job classification systems and who negotiates them. In all countries, there is a tendency for better qualified and more skilled workers, usually men, to take positions of responsibility within trade union structures. Regardless of their formal and legal representative status, post-holders and negotiators are often drawn from the most privileged sections of the labour force. In this respect, their involvement in the negotiation of classification grids contributes to the perpetuation and reproduction of systems of job hierarchy insofar as these are not actively challenged.

CONCLUSION

This research project, in focussing on the workplace rather than on higher level institutions, allowed the practice of the actors to be investigated rather than the institutional and normative framework in which they operate. In focussing on a specific area of policy intervention, it was illuminative of industrial relations

practices. It allowed certain similarities in outcomes to be identified, which raised basic questions about the reproduction of disadvantage in the labour market. The juxtaposition of different systems of industrial relations and union practice required more searching questions to be posed concerning the nature of representation and the ways in which solidaristic ideologies are implemented. We believe that there are general lessons that can be learned from this experience relating to procedures for developing a comparative methodology. Unfortunately, this research project was never funded beyond the pilot stage due to the changing priorities of the European Union, although some bilateral collaboration has been developed between the members of the five national teams.

In procedural terms, the methods adopted in this project contributed to the development of a comparative methodology which went beyond the juxtaposition of expert analyses of national contexts. This involved the exploration of concepts and a process of familiarising all the teams with the different national contexts in the early stages of the project. Significantly, it was through case studies that practice was examined. Moreover, the crossed interview technique constituted a particularly powerful mechanism for developing comparative study. Although one of its purposes was to ensure a degree of comparability in the different country studies, its greatest strength lay in the way in which it allowed both 'insider' and 'outsider' perspectives to be developed. In terms of the research focus, it allowed new questions for research to be identified. More importantly, it contributed to mutual understanding of different national realities by the team members: the nature and meanings of concepts such as a trade union, union membership and collective bargaining in each national context were appreciated. In this respect, as Hyman has argued, "cross-national comparison forces the observer to address critically what is normally accepted as unproblematic within the individual national context: what is otherwise taken for granted is shown to be contingent and perhaps exceptional" (1994: 2).

Although there appears to be growing scope for developing international projects through the EU programmes such as Leonardo, we are not optimistic about the possibilities for developing comparative research within this framework. This is because funding cuts to the research programmes have the result of reducing the resources overall and, in particular, the effort put into the early stages of exploration and development of a common methodology. This means that economies are made in the process of deconstructing and problematising research questions and this seems likely to promote approaches based on the juxtaposition of expert analyses based on specialist knowledge rather than comparative methodology as we have defined it.

NOTES

1. The project was initiated by the Institut de Recherches Economiques et Sociales (IRES), Paris, and focussed on the question of 'les bas niveaux de qualifications' (workers with a low level of qualification) which was a category of workers and identified the discourse of French public policy. The initial stage of the project was financed by the French Ministère de la Recherche and brought together five teams from the Istituto de Richerche Economiche e Sociale, Rome; the Centre d'Economie et Sociologie Regionales, Université Libre de Bruxelles; the University of Tübingen, Germany; the Industrial Relations Research Unit, University of Warwick; and IRKS, Paris. This allowed an initial meeting to take place in Paris at which the different national contexts were presented and debated and a common research methodology was defined. The findings of the first stage of the project have been published in a special number of the *Revue de l'IRES*, September, 1993. A second stage of the project was funded by the Poverty III programme of the European Commission. This was envisaged as a pilot stage, although the teams were not able to obtain funding for the next stage of the project. The findings of the pilot stage were submitted as country reports to the European Commission.

2. The conference 'Education/travail: un champ de recherche dans trots pays européens: Allemagne, Italie et Grande Bretagne', held in Paris in March, 1994, attempted to identify the main currents of research in this field in these countries in order to establish an 'intellectual cartography' which could improve communication between social scientists at European level.

3. The industrial relations glossaries have been published by the European Foundation for Living and Working Conditions, Dublin.

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INTERNATIONALIZATION AND DOCTORAL STUDY: SOME REFLECTIONS ON CROSS-CULTURAL CASE STUDY RESEARCH

Ilse Beuselinck

Editors' Commentary: *In this chapter cross-cultural is defined not in terms of differences between countries, but in terms of culturally specific differences within one country. Drawing on a study of postgraduate education and training across the different language communities, which comprise the country of Belgium, Beuselinck discusses the rise of English as a global scientific language, which in the context of cross-cultural research teams obviously benefits English speakers. However, her concern is not just with language as a mode of speech, but also with its religious and political implications. In the Belgian context, where three different language communities exist within the one country, Beuselinck demonstrates how such implications may pose a problem for the cross-cultural case study researcher.*

This chapter will focus on methodological issues surrounding the conduct of cross-cultural research. The discussion is based on our experience of researching postgraduates across different countries within two international research projects. A team of researchers drawn from across the European Union carried out the first project, by order of the DGXII of the European Commission. This study focussed on opinions and experiences of working scientists and postgraduate research students concerning the desirability and

feasibility of the internationalization of research training in Europe. It was concerned with the way in which opportunities for internationalization can best be taken and the value of existing collaborative structures. A central aspect of the study was to consider barriers to further internationalization (Blume, 1996: 3). The second project, funded under the European Union Human Capital and Mobility initiative, also considered postgraduate education and training in higher education institutions in the European Community.

In both cases, we gathered the information and carried out the case-studies in relation to the situation in Belgium. Whilst the discussion in this is illustrated with examples from Belgium, we also use examples of cultural differences within Belgium and examples of cultural differences among the different European countries involved in the initiatives.

The chapter is composed of two parts. In the first, we will focus on the question of what is meant by cross-cultural case-study research both generally and in the context of our specific projects. The second part deals with some concrete methodological considerations emerging from our own experiences with international research. We will shed light upon four issues: (1) the general conduct of the study; (2) aspects of research design and access to the field; (3) the phase of gathering the data; and (4) the interpreting/analyzing of data. It is not our intention to develop any theoretical orientation or research model but merely draw attention to some obstacles we experienced.

WHAT IS MEANT BY CROSS-CULTURAL CASE STUDY RESEARCH?

In order to point at methodological issues underpinning cross-cultural case study research, in the first instance we should have a clear definition of these different concepts.

Starting with the concept of 'culture' it is important to look at new scientific approaches to the way one conceives culture and society. Whereas culture earlier referred to the best achievements of a civilization, the concept has now been taken to refer to a way of life or outlook (Alasuutari, 1995) adopted by a community or social class. The formerly predominant hierarchic notion has been replaced by a rather 'profane' definition of culture. Examples are the studies of the Birmingham Center for Contemporary Cultural Studies on youth and working-class subcultures (e.g. Willis, 1977). Culture, from this perspective view, can be defined very broadly and even refer to, e.g. social class, sex, age cohorts and also regions and nations. We use the term 'culture' to refer to the different European countries involved in the projects.

Searches of the existing literature reveal, a considerable number of publications about cross-cultural research. These tend, however, to be based, predominantly on psychological and anthropological studies which aim to analyze foreign non-western cultures in an ethnographical way. From this assumption, we can hardly classify our research project about postgraduate education in the European Community as being cross-culturally. It would however be absurd to deny that there are differences between countries of the so-called Western culture. These can surely be analyzed by using cross-cultural paradigms. As growing internationalization together with European integration force research-teams to cooperate on an international scale, it would therefore be more useful to apply the term 'cross-cultural' to all kind of international research instead of preserving it for rather exotic ethnographic research. Surely, as Brislin (1993) remarked

> this trend towards international collaboration will accelerate since multiple-nation and co-authorship serves as a check against ethnocentric biases of research strategy and data interpretation (Brislin, 1973: 8).

Moreover, anthropologists who study foreign cultures complex societies in remote places have to face the fact that time and place have lost much of their meaning in the present world. The 'others' have moved next door and the western artefacts like television and economic networks have invaded practically the entire globe (Alasuutari, 1995).

Finally a case study consists of a detailed investigation whereby the phenomenon is not isolated from its context but is of interest, precisely in relation to its context. Therefore a case study is not a method but rather a research strategy. Qualitative as well as quantitative methods may be used. Consequently we should define a case study in terms of its theoretical orientation which is the emphasis on understanding processes alongside their context (Hartley, 1994: 208–210). The case study as a research strategy has been used widely within sociology, industrial relations and organizational studies. We use this strategy in order to study the phenomenon of postgraduate training. Of course, specific problems will arise when case studies are carried out in a cross cultural setting. We will now consider some of these problems.

SOME METHODOLOGICAL CONSIDERATIONS

General Conduct of the Studies

Both studies we refer to were conducted by a team of researchers from the social sciences drawn from the different countries involved. In fact, each

researcher analyzed the situation in his/her own country, based on broad research questions developed at preliminary meetings, where all of the countries were represented. We did not start with a rigorous set of criteria or research plan but worked with general guidelines. These enabled the different researchers to carry out the research in their own country and its specific cultural context, but at the same time ensured reasonable similarity between the different inquiries. As each researcher stayed in his/her own country we had the advantage that the researchers were familiar with the specific setting and were able to study the phenomenon in depth, knowing the specific cultural meaning systems which is a focal point in case studies.

At meetings with the researchers, each explained the organization and context of postgraduate study in his/her country as clearly as possible to the rest. This provided a starting point for the comparative case studies. Here however we can ask the question whether it is possible to fully understand the situation in another country when one is not a member of that country and has not visited the country or spoken to the interviewees himself/herself. This I think is a major disadvantage of the way we worked compared to cross-cultural research whereby the same researchers visit the different countries. Two issues seem to be important. First, as already said, to make a useful comparison, one should know each culture/context in depth and secondly researchers should, in order to fully understand each other, speak the same language. Both aspects may have a disturbing influence during the different phases of the research project. We should add that both items could also occur as a problem within one country e.g. the language barriers in Belgium. An issue which we will pick up later in the text.

We will now consider some problems, which may occur within the different phases of the research. Moreover, I would like to argue that most of the problems described here only become visible during comparative work and with the presentation of the material to others after completion of the case studies.

RESEARCH DESIGN AND ACCESS TO THE FIELD

Within an international research project, research questions should be relevant within the specific context of each participating country (Brislin, 1973: 13–14). Earlier many authors pointed out the importance within a cross cultural setting of a well-defined topic. General issues are mostly too abstract, complex and multidimensional that they are of little use and intersubjective verifiability is lost. My experiences with international research are that within a well defined area or topic, it might be necessary to start with broad and rather vague research

questions which can be specified during the research process and allow some flexibility to each researcher, as each situation can be quite different. For instance in our second project it was the intention to conduct a comparative study of postdoctoral training programs. We quickly found that in several countries including Belgium there (depending on the scientific field) simply does not exist a training program for postgraduates or, the program exists only on paper and not in practice. We then agreed to look at the general process of completing a Ph.D. with or without a formal training program.

We choose to study faculties of engineering. This gave us access to a scientific field which is well developed in all countries and is strongly research-oriented on an international scale. New developments follow an international path and are less influenced by social and historical conditions, as for instance in the humanities. Furthermore, in most faculties of engineering in Belgium, a training program exists. We thus felt it would be a rewarding choice for comparative research. One could of course argue against this choice, saying that whilst we have chosen to study those aspects of the field that are easy to compare, they might not yield the most interesting issues, or those which lead us to fully understand the differences in the field and to develop new theoretical orientations based on cultural diversity. While there is no reason to deny this criticism, it is, however, important to make the most of what one has. Until recently, the field of postgraduate research training has been a neglected field of study. It would seem better, therefore, to start with an exploration of similarities and to focus in depth on more complex and hidden aspects in at a later stage.

Within the first project about mobility among postgraduate students guidelines were intended to ensure reasonable similarity between the different case studies (Blume, 1996: 4). It was thus agreed that in each country attempts would be made: (1) to visit at least two institutions: one large metropolitan university and one provincial university; (2) to cover one field from the natural/ technical sciences and one field from the social sciences/humanities; and (3) to interview both senior scientists and students themselves. In our Belgian case, we felt it would make more sense to select a Flemish and a Walloon university instead of a metropolitan and a provincial one. Firstly because the difference between a metropolitan and a provincial university does not really exists in a small country such as Belgium. Almost all universities are located in cities and we cannot speak of a central and a peripheral area, as is the case in for instance France. Secondly, Belgium is characterized by a much more fundamental division, namely that between Flanders and Walloon. Both communities differ in more than one way. There is of course the language to begin with but it is also important to note the different cultural identity. Furthermore, both

communities have a different religious and political background. Whereas Catholicism is deeply rooted in Flanders, which is reflected on the political plan, Wallonia is characterized by a Socialist tradition. All these aspects lead to the development of a federal state in, 1989,[1] a complex political compromise which, even for many Belgians themselves, is not transparent.

Clearly this deviation of the general outline of the study is based on well-reflected considerations and undeniable cultural-historical aspects of the country. However it makes comparative research more difficult, requiring findings to be interpreted within the light of these different points of view.

THE GATHERING OF DATA

I would like to discuss two major issues in this part of the chapter. (1) Language barriers; and (2) the respondents attitude towards the study.

Amongst the group of researchers the language used was English. However, as already stated, there was a language barrier within the group. Consequently, we could pose a fundamental question as to whether each researcher fully understood the global framework of the project. It is also possible that the different participants interpreted the same aspects of the study in different ways. As this happens within research-groups wherein each member speaks the same mother tongue, it is certainly possible within a group of different nationalities, where not everyone shares the same level of speaking and understanding the chosen language of communication. Needless to say, native English speakers have an incontestable advantage on the scientific platform. They are not confronted with the problems of translation into English, which may severely slow down the scientific writing process. However this is a general problem occurring at each phase of the study. For example, when looking at gathering the data, the group should be confident that questions for interview written in one language are equivalent in meaning to those in another.

Moreover, language can pose a specifically problem during the data collection even within one country. For example, as Belgium consists of different language communities, interviews had to be conducted in Dutch and in French. Being a Dutch-speaking researcher, the interviews taken in the Walloon part of Belgium was a tall order, demanding conscientious preparation. Although Belgians are supposed to be bilingual, it remains a difficult task if one is not accustomed to use the other language daily. On the one hand, it is especially difficult for the researcher to immediately react to certain statements made by interviewees or to elaborate specific topics which rise unexpectedly during the conversation. On the other hand, it might also be

difficult for the interviewees. Listening to people not speaking their mother tongue may be fatiguing, and demanding extreme concentration. This may lead to a situation whereby the attention of the subject is lessened and communication problems are so severe that the researcher is unable to communicate anything at all to his/her interviewees. It is however difficult to eliminate language-barriers. We cannot introduce experimental control groups in order to check the validity of the data as it may be possible say with, research in the field of experimental psychology tests.

In addition to language, the attitude of the respondents can affect the gathering of the data (Brislin, 1973: 1–32). Cross-culturally there may be a sample of very cooperative subjects in one country and rather suspicious subjects in another country, leading to slightly different data sets. This variety in samples may be due to different causes and it may be necessary to recognize that there may be a difference between the respondent's general attitude towards the project and their attitude towards the interviewer. Whilst these are issues which are not specifically related to cross-cultural studies, but occur during almost every research project. However, again indirectly they may affect the general comparative work, delivered by a team of researchers. It is important, in order to interpret the data of each country, to know all problems, even the smallest, that occur in each individual case-study. The following two examples deal with the respondents' attitude firstly towards the project in general and secondly towards the researcher.

In both research projects about post-graduate training, I needed to interview Ph.D. students and professors from physical science and engineering. Being a social scientist, on each occasion I was confronted with people working within a totally different scientific paradigm from the one with which I was familiar, namely the exact/positively based scientific approach. I had the feeling that some of the students and professors could not identify themselves with the kind of social research in which they were asked to take part. Whilst it would be wrong to say they were suspicious or they did not cooperate, on the contrary, no one refused to cooperate, I felt a general attitude of some disbelief towards the scientific value of this kind of research, where they were invited for a 'pleasant chat' about their personal experiences.

Besides this rather vague and hidden attitude of disbelief, I should also mention that some respondents made me feel inferior towards them. At this point I'm not sure whether I felt inferior or some respondents felt superior. I suppose there is an interplay of factors, which strengthen one another.

Berg (1995: 50–55) argues that actions, lines, roles and routines must be carefully prepared and rehearsed in advance by the interviewer in order to constitute a self-conscious performance. Therefore, the researcher uses the so-

called 'interviewer's repertoire', a set of devices (making adjustments throughout the interview, consisting largely of switching from one role to another or altering their style of speech, manner or set of lines) in order to collect as much information as possible. In other words the researcher is constantly trying to portray a convincing characterization of his/her role (see also Goffman, 1967). Here I think it is possible that the researcher is hindered by his/her other societal roles. For example, I am a researcher, I am also a 26 years old female sociologist who is questioning, in almost every case older male physicists and engineers including some professors. Not only age and sex, but also the scientific field contributes to the informal and implicit status system operating within the research situation. It is notable that I always felt most at ease while talking to young female scientists, whereas the interviews with older male professors tended to be rather more formal. Especially during interviews with professors, I had the feeling that roles and the fundamental status hierarchy were inverted as I asked the questions. Moreover, I was afraid of posing questions which seemed 'stupid' to them. Other researchers of the wider group (for both projects) did not mention such problems. Often they were older, could rely on more experience in the field or had already obtained their own Ph.D., which assured them of a similar academic level to that of the professors being interviewed. In conclusion, I should also stress the fact that, compared to other countries, gaining a Ph.D. in Belgium or belonging to the university academic staff, remains an elitist status, predestined to a small minority. This culturally based social meaning, attached to the profession of academic staff-member might also influence my research within these institutions, facilitating the collection of data in a general sense. While I as a member of this culture, automatically play 'second fiddle'. Consequently, it is not merely the skill of the researcher that influences the success of the interview. Whilst the researcher tries to control the interview process in a reflexive way using all kinds of strategies, there may be many other background issues which dominate the situation.

THE INTERPRETATION AND ANALYSIS OF THE DATA

We have already mentioned the issue of culturally based interpretation of the data several times in this chapter. While carrying out a case-study, a focal point is the importance of meaning and the mediation of social life through meanings (Alasuutari, 1995: 26). The advantage of our studies was that each researcher could work in his/her own country and consequently could interpret the data within the necessary context. It thus is important that the different researchers understand each other while comparing the cases. The following example from

a study of mother-child relations, (Wesley & Karr, 1966) shows a possible problem for data interpretation in such circumstances.

> If for instance, a German and an American mother are both asked how they would punish misbehavior, both may give the same answer. However the German mother may have interpreted 'misbehavior' to mean 'being ten minutes late for dinner', the American mother perhaps as 'not coming at all'. The mothers might have given entirely different answers had the misbehavior been specified to mean 'ten minutes late for dinner'. Mothers of two cultures may answer one and the same question with, for instance, 'moderate punishment'. By 'moderate punishment' the mother of one culture may mean a verbal scolding, the mother in the other culture a physical punishment such as a slap in the face (p. 260).

The international studies of postgraduate education revealed similar things. For example, in the first project focussing on international mobility among doctoral students, all interviewees agreed that mobility is valuable. However superimposed on this general view, are a number of more subtle differences reflecting national cultural traits. For example, it was claimed that the Irish have always been great travelers and it was seen as part of traditional Irish culture to be mobile. Historic-linguistic relationships were also seen to be relevant, (e.g. France with Francophone Africa, Spain with Latin-America) and traditions specific to higher education practice such as the longstanding encouragement of mobility in some Nordic countries (Blume, 1996: 6). In other words, though everyone was generally positive about academic mobility, the meaning and significance attached to it may be strongly mediated by certain aspects of social, cultural and historical conditions within each individual country.

Also more practical and legal arrangements concerning the organization of doctoral training programs in the different countries, may lead to diverging perceptions. Talking to one of my colleagues of a southern European country, we found out that, compared to the southern European countries, postgraduates in Belgium have a well-developed legal statute. They are contractually employed at the university and receive a monthly wage. Totally different is the situation in the southern country in question where Ph.D. students are statutory students. Consequently they have no legal income and are, as long as they work on their Ph.D., forced to live with (in most cases) their parents. Such a comparison immediately evokes feelings of social inequality within the European Community, wherein some people, though doing the same 'job', depending on the country in which they live, benefit from better legal arrangements than others. However while discussing the issue in greater depth, it appeared that this previously found social inequality, could in some instances be overridden by important differences concerning later labor market opportunities. While job opportunities for doctoral graduates within Belgian

academic system are extremely small, the academic profession automatically takes those obtaining their Ph.D. in the discussed southern country up. In this context, remaining within the statute of a student can thus be interpreted as an investment for the future, which would not be the case in Belgium.

Concluding, we could say that every culture should be understood in its own terms. However, at the same time, another goal of cross-cultural case study is to incorporate aspects of many cultures into a general hypothesis or theory. Brislin et al. (1973: 24) here use the emic-etic distinction derived from linguistic terminology.[2] Cross-culturally it is important to build theory based on comparative research, which is called the etic part of social research. However one should always be aware of the danger of imposing an artificial etic and losing the emic or meaningful aspects of the other culture as practiced by their members. Therefore, research instruments should include items aimed at both etic and emic aspects. The examples given of the study about mother-child relations point out the problems of imposing an American-based etic without looking at the emic meaning behind German responses. A possible outline of cross-cultural research is given (Brislin, 1973: 25). First, it is important to start from emic meanings. At the next step, researchers can look for those aspects of behavior that are functionally equivalent between the different cultures. Descriptive categories derived from past research, perhaps in only one culture, can then be applied to the other culture (this would equate with a notion of imposed etic). Of course these imposed descriptive categories should be adapted so that they explain the cultural practices of the other culture from their point of view (i.e. emic description). Finally these shared categories can then be used to build up new categories valid for both cultures (derived etic). While it is difficult to follow this outline point-by-point, it nevertheless offers a useful support when analyzing data in an international context.

In our first project about the internationalization of research training, we discovered some interesting (etic) categories that are valuable in the different countries which were studied. One of the most striking findings of the study was for instance the very different processes underlying mobility in the natural sciences and the social sciences (Blume, 1996: 28–30). Students from the natural sciences often have trips of short duration, enjoying institutional sponsorship. In the social sciences on the other hand, students frequently invest considerable time, personal resources and effort in organizing trips for themselves. Too often this appears to be a frustrating business, in which home institutions and departments offer little or no support. Regardless of the specific social and cultural context of the participating countries, these shared differences emerging between the sciences can be related to the degree of collectivization of research. Where research is largely collectivized, e.g. in

natural sciences, the skills and information which students bring back on return from an international visit are also collectivized. Colleagues and the research in general can profit from their experiences. In the social sciences by contrast, research is much more individualistic. Colleagues can rarely profit from the skills and insights which a returning Ph.D. student brings with them. This analytical distinction also explains why there seems to be no unanimity concerning the question of what precisely the experience of going abroad should offer the student. Some students emphasize the acquisition of social and linguistic skills, the challenge of working in a different environment, becoming a mature scientist etc. while others assessed the trip in terms of its contribution to the speed of completing their thesis or project.

Our research suggests, there were two 'logics' which play an important role in the context of internationalization of postgraduate research study. Namely the logic of personal development and the logic of effective completion. This second logic coincides with the first, in that students of the social sciences, where direct contribution to a collective activity is absent, are more likely to seek the longer-term personal benefits of personal growth, while students of the natural sciences speak more in terms of completing the thesis and contributing to the general development of the research unit.

CONCLUSION

This chapter has attempted to shed light upon some possible problems when carrying out cross-cultural case-study research. It was not our intention to give a complete outline of all problems that may beset such an approach. This, I think, is not possible as each research design needs a different approach and researchers will, dependent of the theme and the parties involved, be confronted with different problems.

Though we tried to outline the problems according to the different steps taken in a study, this is of course a simplification of reality, as some of the issues discussed cross each research phase. Our research suggests that some of the most important issues facing the cross-cultural case study researcher, are the language barriers and the different social meaning systems within the participating countries. Obviously both items are interwoven. If researchers are, because of language barriers, severely inhibited in their communication with each other or with their interviewees, it will be much more difficult to obtain a convincing image of underlying meaning systems, which influence habits and practices within each country. A necessary strategy therefore is to be constantly aware of these possible pitfalls and to plan enough meetings with the whole group to ensure that each individual researcher maintains a sense of the

whole project and that common experiences and concerns, especially within the context of cultural differences, can be shared.

NOTES

1. Since then, Belgium is drawn up of Communities and Regions. There are three cultural Communities: The Dutch-speaking (Flemish) Community, the French-speaking Community and the German-speaking Community. Each Community has authority over the corresponding language community. There are three Regions namely the Flemish Region, the Walloon Region and the Brussels Region.

2. In linguistic terms phonetic notation is meant to be a general syztem which can describe all sounds in all languages, while phonemic is meant to describe sounds that are meaningful in a given culture.

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CASE, CULTURE, CURRICULUM AND EDUCATIONAL CHANGE: REFLECTIONS ON CROSS-CULTURAL CASE STUDY IN ENGLAND AND JAPAN

Christopher J. Pole

Editors' Commentary: *In a chapter which discusses the process of team based cross-cultural case study located in England and Japan, Pole examines issues of protocol and etiquette which structure the conduct of case study research in this context. In particular, the chapter focuses on key social variables of gender and age within the context of team based research and examines the way in which these may be interpreted differently in different cultures. The chapter introduces the notion of pace in the research process and relates this to particular cultural characteristics, which may mitigate the detail demanded of case study research. The chapter also examines issues of translation and language codes, where things left unsaid can be as important as those that are said.*

CROSS-CULTURAL CASE STUDY AND CONTRADICTIONS

At one level, the idea of a cross-cultural case study might appear contradictory. Whilst traditional approaches to cross-cultural research place a primacy on the

capacity for comparison across societies, communities and cultures in an attempt to highlight their differences and similarities, case study research has usually been concerned with the detail of one case. Indeed Bouma & Atkinson (1995) go so far as to say

> The key element in a case study is that one group is focused on and that no comparison with another group is made. (p. 110).

Moreover, cross-cultural research has tended to draw primarily on the quantitative tradition of social investigation where units and processes for comparison have been easily identified and data collected in such a way as to facilitate the relatively straightforward comparison of, if not exactly like with like, then broadly similar with broadly similar. In the meantime, case study has emphasized the importance of thick (Geertz, 1973) description and the holistic perspective (Agar, 1980) where the detail of the individual case and the meaning that can be attached to 'one' (Wolcott, 1995) has provided the rationale for the approach. Moreover, where cross-cultural research has tended to focus upon structural aspects of community or culture, case study, whilst not astructural, has given more attention to processes, interaction and the social construction of meaning and significance.

   The principal location of this apparent contradiction would seem to rest with issues of scope. Where cross-cultural research has tended to look across and between cultures, case studies have looked in depth and detail. For some, the case study has lacked a clear definition, being synonymous with general qualitative method or ethnography (Hammersley, 1990). For others, case study has lacked rigour and precision. Consequently its capacity to offer explanation and understanding of social phenomena has been severely challenged. Campbell & Stanley (1963) go so far as to state.

> (The) 'One-shot case study', which is one way of describing much qualitative research, has 'such a total absence of control as to be of almost no scientific value' (Cited in Ward Schofield, 1993: 204)

In the context of cross-cultural research, such a dismissal of case study offers a particular challenge to the researcher concerned to produce a synthesis of breadth with detail. However, the challenge does not represent an en passe. Its resolution would seem to rest on definitions of culture and case.

Culture

In this volume we have seen culture defined in a number of ways. Verhoeven has talked about culture in terms of worldview, which includes issues of meaning and shared understanding. Coffey and Atkinson have discussed

culture in the context of different intellectual cultures which characterize ethnographic approaches to research, whilst Broadfoot and Rainbird et al. discuss the importance of language and shared meaning to the conduct of research in different countries. In this sense, the contributors provide a definition of culture which takes us beyond the physical dimensions of geography to incorporate culture as a social, political and historical phenomenon. In doing this, they illustrate several of the characteristics of culture included in the definition offered by Abercrombie, Hill & Turner (1988) who see culture as:

> The symbolic and learned, non-biological aspects of human society, including language, custom and convention, by which human behaviour can be distinguished from that of other primates (p. 59).

Whilst in itself broad and inclusive, this definition allows the cross-cultural researcher to narrow the focus of his/her research in order to concentrate on any one or more of these 'non-biological aspects of human society'. Cross-cultural research need not entail an all-embracing, panoptican approach, but can focus on elements of a culture which contribute towards a bigger picture and allow a degree of comparison between the locations in which specific cultures might be located.

Case

Burgess (2000) in this volume invites us to consider the location of the 'case' in any case study. His assertion that the case may be constituted in many different ways, for example an individual actor, a process or an institution, lends itself well to the conception of culture outlined above. In short, a case may relate closely to an aspect of culture defined as a 'symbolic and learned, non-biological aspect of human society' (Abercrombie et al.) However, its principal characteristic is its capacity for holism, in that the case remains a discrete entity (Mason, 1996). Indeed, Yin (1989) asserts that a case study enables the retention of

> (T)he holistic and meaningful characteristics of real-life events- such as individual life cycles, organizational and managerial processes, neighbourhood change, international relations, and the maturation of industries (p. 14).

In some respects, the definition of what can constitute or count as a case necessarily remains 'fuzzy' (Agar, 1980). Nevertheless, whatever is deemed to be the case needs to be carefully specified. In this respect, perhaps self-evidently, the case becomes the focus of case study research.

Culture and Case

By offering these definitions of case and culture, we are able to progress the concept of cross-cultural case study beyond the en passe suggested above. Cross-cultural case study offers the possibility of detailed research into discrete aspects of symbolic and learned aspects of human society in a way that is focused and detailed. In addition, given its emphasis on discrete aspects, it allows meaningful comparison between cultures. Moreover, if culture is defined in terms of an amalgamation of discrete aspects, then within the ambit of the case study, a number of such aspects may be considered simultaneously. In short, this implies that the case study may be concerned with several different aspects of culture at the same time. For example, we may be concerned with language, custom, belief, value systems and more tangible things such as style of dress, etiquette and even methodology within a case study.

   Cross-cultural case study is, therefore, more than a research method or collection of methods. Its focus on discrete holistic phenomena where the emphasis is on detailed description and depth of understanding, locates it within a general paradigm of qualitative enquiry and more specifically with ethnography. Whilst it is an approach based on empiricism, it contrasts with approaches which seek simplistic causal explanation of social process and social phenomena. In this sense, it relies on a form of contextualized empiricism where the case relates directly to a specific element of culture, rather than any form of abstracted empiricism, where data and the case may be seen to exist merely for their own sake.

THE CASE AND THE CULTURE

Having offered a definition of cross-cultural case study, together with some comments, which I hope, will contribute to debate about what it might achieve, I wish to take the discussion further by providing a case study of a case study. Using the example of cross-cultural case study research conducted in England and Japan, my intention is to highlight some of the challenges posed by attempting to research across a number of different aspects of culture and to produce, in this instance, an understanding of the process of educational change.

   The research in question brought together a group of English researchers with a group from Japan. The intention was that each team would conduct a study in the others' country. Funding for the research was secured from the Daiwa Foundation and Osaka Kyoki University in Japan, sufficient to allow

field visits to Japan and England. The intention was to conduct school based research using interviews, observation and documentary analysis, during a period of educational change in both countries. From the outset, the project was seen as challenging, not least due to the fact that the English researchers spoke no Japanese! However, the research afforded the opportunity to study aspects of educational change in what were assumed to be very different education systems in very different national cultures. In this respect, the methodological challenges posed by difference, contributed to the motivation for the research. Like much cross-cultural research, an element of the exotic was part of its attraction.

Identifying the Case

From the outset it seemed possible to identify a number of cases which would provide the focus for the study. Given we were researching in what are popularly perceived as two countries with very different national cultures, (Joseph, 1993) the countries themselves might have provided the case. However, issues of research focus together with the feasibility of the research (Delamont, 1992) suggest that this would not be productive or indeed possible. Whilst some research in the tradition of community studies (Payne, 1993) has a cross-cultural dimension, the idea of studying a country was clearly not feasible, not just in relation to focus and scope, but also human and financial resources attached to the project.

  Employing the principles of progressive focusing (Glaser & Strauss, 1967) we might have identified the two different education systems as the case or cases. However, many of the limitations suggested above also held here. Discussions with our Japanese colleagues often centred on issues of educational change in the two countries, its implications for teachers, pedagogy and the wider relationship between education and the economy. The concept of educational change was one in which both groups had sociological interest and seemed likely to be productive for the study. However, as many educationists (e.g. Fullan, 1993) have shown, change often proves difficult to define and to identify, let alone research in a country where you do not understand the language. Nevertheless, despite such difficulties, it proved to be the driving intellectual force for our research proposal to the funding bodies. In addition, in order to operationalize the research and to identify a case, we needed to consider the technical aspects of the research process along side the intellectual and epistemological. As Bryman (1988) asserts, it is often the technical aspects of research, which prove most influential in shaping its focus and design.

The principal location of educational change in both countries was the school curriculum. After further discussion and preliminary reading around the subject, the curriculum emerged as the case for the research. Notwithstanding debates about definitions of the curriculum (Kelly, 1999, Lawton. 1975), in an effort to further refine the research focus, three subjects, English Language, Science and Physical Education (P.E.), were identified within the curriculum which would be studied in schools in Japan and England.[1]

In this example, therefore, the case operates at a number of different levels. The levels serve to link micro and macro aspects of the research and ultimately allow us to address structural and interactional issues relating to education in the two countries. For example, by taking the specific subject cases of English, Science and P.E. we would be afforded a detailed picture of the operation of the school curriculum in three contrasting areas. Underpinning this specific focus, however, was our principal concern with the impact and experience of educational change. In effect, our research design centred on a multi-level case study where there was a direct relationship between the different levels. In short, collecting data on English, Science and P.E. allowed us to examine key aspects of the curriculum, which in turn we saw as the principal site of educational change in both countries. Educational change remained the case, but it was accessed via the curriculum and more specifically, particular subjects within the curriculum were identified as providing useful examples of change and about which data could be collected.

Identifying the Culture

As with issues relating to case, culture could also be defined at a number of different levels. The most obvious definition related to geography or national culture. However, attempting to characterize culture in respect of nationality could be seen as essentialist. Nevertheless, in a commonsense way, it was possible to identify many cultural differences between England and Japan, English and Japanese people and in respect of English and Japanese way of life. It was also possible to address issues of culture in the context of education and school and also with reference to the case outlined above, in relation to change. From a methodological perspective, it was also possible to identify culture in relation to research. As will become clear later in this chapter, there were significant differences between the English and Japanese teams in their approach to research and how it should be conducted.

Often, definitions and discussions about culture centre on semantics. In the context of cross-cultural research, however, definitions need to be oper-ationalized in such a way as to facilitate the research process. In practice, this

may mean that the cross-cultural researcher works with several different kinds of culture simultaneously. In respect of the definition of culture offered earlier in this chapter (Abercrombie et al., 1988), these may be the 'symbolic, learned, non-biological aspects of human society'. Furthermore, it seems unlikely that the complexities of case and culture discussed here were peculiar to our research. Together with those of other researchers recounted in this volume, they suggest that cross-cultural case study is an approach, which has the capacity to operate on several different levels. Insofar as the notion of both case and culture allow exploration at structural and interactional levels, in which it is possible to locate micro issues in the context of more macro concerns.

THE CROSS-CULTURAL CASE STUDY COLLABORATION, CURRICULUM AND CHANGE

The collaborative nature of our research was important in providing the substantive focus for the case study. Much of what the Japanese team were concerned to study in England focused upon the impact of the 1988 Education Reform Act and in particular they were interested in issues of curriculum, testing and school management under what seemed to be a new and ever changing regime for education.

Our discussions with the Japanese researchers about their work in England and the possibilities of our work in Japan constantly addressed the notion of educational change in a variety of different forms. For example, in terms of the organization of education, the role of the state and the capacity for school and individual professional autonomy. We were also concerned with the different kinds of knowledge which each of the education systems considered essential for their pupils to acquire. Allied to this were question of teaching styles and pedagogy. Our discussions revealed that although the organization of education and schooling in our two countries was, in many respects very different, there were many shared concerns that we had as sociologists of education, based on changes that were occurring within these very different systems.

Whilst the organization of education in England and Wales might be characterized as constantly changing, at least since the second world war, the Japanese system has by comparison been static. Its organization, teaching methods and curriculum remained largely unchanged for a period of more than forty years. Despite constant change in England the scale of the changes introduced by the 1988 Education Reform Act were of a different and much grander order. The Act brought changes to the organization and finance of schooling, the curriculum and testing, introducing what many commentators

saw as a market model for education (Bowe et al., 1992; Pole, 1998; Ross, 2000; Simon, 1988).

At the same time, since 1988, there has been limited change introduced into the Japanese system. In comparison to changes in England this may have been interpreted as small scale. However, the change needs to be seen against the backdrop of forty years of stability. In this context it may be that the magnitude of the change, in terms of the impact in classrooms, in the relationship between school and the state, in relation to teaching as an occupation and to the curriculum is closer in the two countries than a cursory or face value examination would reveal. In identifying educational change as the Case for the study, our concern was not solely with the official aims and objectives and rationale for the various reforms taking place in the two countries, but also in the ways in which change has been received and interpreted by the practitioners, principally the teachers. In short it was the relative impact of change which provided much of the sociological interest for the study.

If we take as a starting point a view that the education system in England has traditionally been a decentralized system and by way of contrast that the Japanese system is centralized, then we have two fundamentally different starting points for any consideration of the scope and impact of educational change. At this stage it will be useful to give examples of what I mean by centralized and de-centralized systems.

In Japan a National Curriculum has existed for many years. Decisions, often, by Western standards, about seemingly trivial or internal school matters are passed down the organizational hierarchy from Prefecture, to Local Education Authority, to school level, leaving little scope for innovation or autonomy on the part of individual head teachers or teaching staff. Moreover, all teachers and head teachers are moved from school to school throughout their career, with little consultation or concern for their own preferences. The average period of time for a head teacher in any one school is four years and the maximum time for a teacher in any school is eight years.

The foundation of the Japanese education system can be directly attributed to the publication, in 1947, of twenty-five items for the 'General Aims of Education' in Japan. These aims were prepared by a Ministry of Education Committee and have remained the cornerstone for the Japanese education system ever since. These aims relate not only to the school organization and curriculum but also a general 'ethos 'of education and its role in the

> rearing of (the) people sound in mind and body who shall have truth and justice, esteem individual value, respect labour and have a deep sense of responsibility (Ministry of Education, 1991).

The centralization leaves little scope for innovation by practitioners, seeking to foster a uniformity of provision and experience. In addition, education is tied very closely to the economic and social aspirations of the country as a whole.

In England, at least up until the education Reform Act of 1988, the situation has traditionally been very different. Education has been administered at a local rather than a national level with much responsibility being vested in the Local Education Authorities (LEAs). Only since 1988 has the content of the school curriculum been enshrined in Law and there have been no explicit, centrally prescribed national or general aims of Education. Many different kinds of schools have existed throughout England and even within individual LEAs. By comparison with the Japanese system and those of many other countries, the organization of schooling in England has been diverse. Moreover, head teachers have traditionally enjoyed considerable autonomy with their schools in matters relating to curriculum and staffing and school organization. Within the classroom, prior to the National Curriculum, teachers, particularly at Primary level, have also enjoyed autonomy not only in what they taught but also how they taught it.

The Scale of Educational Change

In relation to our cross-cultural case study of educational change, the characteristics of the two education systems outlined above have particular significance. They illustrate different starting points for the study of change and in doing so, suggest that its impact may reflect the situation prior to its introduction, as much as the real or tangible differences which it brought about. At this stage it is useful to consider a summary of the principal educational changes introduced in England and Japan at the beginning of the 1990s.

In the English system the 1988 Education Reform Act introduced the following changes which began to take effect in the 1990s:

- A National Curriculum with clearly specified subjects.
- A system of centrally devised and administered national tests for all children at the ages of 7, 11, 14 and 16.
- Open Enrolment, giving parents greater choice over which school to send their children.
- Local Management of schools in which head teachers and school governors were given greater autonomy in relation to school budgets. Consequently reducing the role of the LEA.
- The introduction of Grant Maintained schools which received funding directly from the Department for Education, thus bypassing the LEA.

In the Japanese system: The reforms introduced through legal measure initiated by the Ad-hoc Council on Education (1988) and the Ministry of Education (1990) are in many respects less tangible than in the English system and relate to

- Greater emphasis on the individual pupil in terms of learning needs.
- Greater emphasis on the role of pastoral care and closer links between school and home.
- Greater capacity for schools to interpret the curriculum according to their own philosophy and the needs/interests of their pupils.
- More active approaches to learning e.g.: more experimentation in science, more conversation in English.
- Less emphasis on testing.
- Greater capacity for pupils to choose subjects in High school.
- Introduction of boys to cooking and health education, and girls to woodwork and other practical subjects.
- An emphasis on internationalization.
- A greater emphasis placed on acquisition of skills. Less emphasis on learning facts (cramming).

Whilst there are material differences in the nature of the changes their analysis in the context of the cross-cultural case study and against the different traditions or histories of education in the two countries allows us to focus on educational change in a conceptual as well as an empirical sense.

In Japan the changes signal a move from control by the centre to greater autonomy for individual schools. There is greater flexibility in the curriculum and in teaching methods, which suggests an enhanced role for teachers and head teachers in shaping and managing educational experience. In England the changes are indicative of tighter control at the centre, more emphasis on standardization in terms of knowledge and pedagogy as a result of national tests. In some respects the financial reforms appear to do the opposite by giving more autonomy to head teachers over budget management. However, closer examination suggests that local financial management is also about bypassing local democracy (Simon, 1988) in the guise of the LEA which may in practice be a further example of centralization.

Interpreting the Changes: Convergence

The cross-cultural analysis of educational change enables us to describe the nature of change within England and Japan. More than this, however, the analysis of change within the specific cultural contexts of the different

education systems, and also within the context of national culture, allows us to locate the change conceptually. Our case study suggests that we are moving, albeit slowly, towards similar kinds of education systems in the two countries. Furthermore, it is interesting that if convergence is taking place, then it is occurring from very different starting points and via measures which in some respects operate in opposite directions. What the cross-cultural case study also shows is that the mechanisms or the structures by which change is being effected share certain similarities.

Both sets of changes are initiated by central government. Both are being introduced in relation to a rhetoric of changing educational structures to the benefit of the pupil and parents. Both countries have identified forms of essential learning either through a core curriculum (England) or a greater emphasis on transferable skills (Japan). Underpinning both is a notion of better service to industry. In the case of the England a better educated workforce. In the case of Japan, greater flexibility in education is looked upon to bring a greater degree originality to industry and commerce.

Our cross-cultural analysis not only suggests that the changes sketched out above.[2] are indicative of convergence in the education systems in Japan and England, but it also offers some insight as to how change has been achieved within the different organizational structures. Moreover, the study with its emphasis on educational change allows us to speculate about the future of the two different systems and the capacity for lasting change. Archer (1979) in her framework of analysis for educational change, itself developed as part of her own cross-cultural work on the development of educational systems, discusses the role of teachers, their unions and other education professionals in bringing about educational change within the context of centralized and de-centralized education systems. Although there is not space to conduct such analysis here, our experience of cross-cultural case study suggests it is an approach which accommodates the application of Archer's framework, not least because of its emphasis on comparative data.

Methodological Considerations (Not) Knowing the Language

The research was of course complicated by the fact that none of the English team spoke any Japanese. This was significant in that our research was to be qualitative in nature relying principally upon interviews and observation for data and where possible upon documentary analysis. In many respects, the most important methodological issue that underpinned the research was how do you conduct research that relies on verbal interaction both in terms of

interviews and to a large extent in the context of observation, when you can't speak the language?

In short, there were a number of strategies which we used to overcome the language difficulties. For example, we used translators whenever possible. For the most part this proved to be effective, or at least we believe so, as checking on the accuracy of the translation we received would be extremely difficult.[3] However, translators were expensive and they tended to vary in the amount of detail they provided. Ironically, we considered the best translator to be a Welshman fluent in Japanese having lived in the country for ten years. Not only did he provide a literal translation, but he also offered some commentary on some of the micro-politics of what was being said. Nevertheless, with all translators we were concerned not only about the issue of literal translation but also the cultural translation and whether the translator would convey the same sense of the line of questioning and discussion. In addition, translation almost doubled the amount of time required for an interview.

In addition, we also had reservations about the dynamics of the interview situation, which would inevitably be altered by the presence of the translator. We feared the presence of the translator could affect the nature of responses to question from the researched. Whilst interviewer effect is a concern in any interview situation, we feared that having no knowledge of Japanese would make judgement of its impact, through facial expression, hesitancy, etc extremely difficult. However, the presence of not only the translator in the interview, but also a whole host of other people superseded this concern. Whoever we interviewed regardless of seniority at school, LEA or Prefecture level there was always an entourage present. This concerned us at first, not least because it was so different from most interview situations we had encountered in previous research. The presence of a group of people had implications for the development of rapport and the extent to which it was possible to tackle sensitive subjects? What was more, we rarely knew who the people were. Introductions at the start of sessions often meant little at the time and often, part of the interview became concerned with establishing the different roles of those present. However, it was not only the case that people were present during interviews, but they often contributed to the discussion. This made translation difficult, as on occasions it was clear that a dialogue was occurring between the Japanese speakers which we were not party to.

On discussing our concerns with our Japanese colleagues we were told not to worry 'It is the Japanese way'. A phrase that was repeated on many occasions during our research in Japan.

The second strategy we adopted to overcome our language deficiency was to focus our attention upon particular aspects of the Japanese school curriculum

and organization, which we felt, would maximize our chances of being able to collect good observational data. Not only did this entail focusing on English, P.E. and science lessons, but also on other aspects of the school day. For example, assemblies, lesson change-overs, after school clubs, lunch time and staff rooms all presented useful opportunities for the collection of observational data. In addition, they also presented issues which could be pursued in interviews.

Pace

The famous Japanese work ethic (Dore & Sako, 1989; Joseph, 1993) seemed also to underpin their notion of research. Research was conceived in terms of doing things; being in different places, seeing different people, to be constantly on the move. Whilst such an approach was to some extent necessary because of our limited amount of time in the country, our impression was that this momentum was in many respects part of the every day work order. But conscious also of what one of our translators told us, that although Japanese men stay at the office until 9.00 PM there is often much more activity around the coffee machine than anywhere else, we began to realize that a considerable amount of our time was taken up with the ritualistic ceremonies; the exchange of business cards, taking tea, a tea ceremony, speeches of welcome and farewell, lunch, more tea. Nevertheless, it was also clear that ceremony was an important aspect of Japanese culture which not only were we obliged to observe, but was also a useful source of data.

However, the pace of the research was demanding to the extent that we feared we were losing control of our programme. We became concerned that we needed to try to recapture some control, not least by having time to discuss and reflect upon the data we had collected. In this we were only partially successful. The schedule of visits, interviews and social events arranged before our arrival made any major changes to the research programme difficult to achieve without causing offence to our Japanese colleagues. Despite our concerns, we succeeded in collecting a considerable amount of data which met our original objectives for the cross-cultural case study.

Hierarchy: Age and Gender

In our preparation for our visit to Japan we had thought about the research in terms of team work in which there would be an equal division of labour based on the three different subjects. The team consisted of three members, two

female and one male, all of whom had the status of Research Fellow at the University of Warwick, U.K. Whilst we had no notion of a hierarchy within the team, our Japanese colleagues did!

Soon after our arrival in Japan it became obvious that the Japanese require a designated leader for many aspects of social life. They assumed, therefore, that our team would naturally have a leader. When we explained that we didn't have one, it became clear that they would appoint a leader.

Within Japanese society it would seem that status is often attributed by virtue of sex and age with sex being the dominant criteria. As a result as the only male member of the team, though not the oldest, I was looked upon as the defacto leader. The perceived need for a leader soon became apparent and related to the various ceremonial duties discussed earlier. Someone to take the lead in the exchange of business cards and gifts, to shake hands with the Japanese 'leader' in obligatory photographs, to respond to the speeches of welcome and to make public declarations of thanks and gratitude on our departure. With an obvious need to be accepted in the various settings and a need to avoid embarrassing our Japanese research colleagues we felt a need to go along with this, at least in the early stages. So I duly obliged with speeches in front of the whole school stating how pleased we were to be there and how grateful we were for the hospitality shown to us, which was never in short supply. As the research progressed, however, we felt more confident, and began to challenge the order. In particular by looking to the youngest member of the team, also a female of Indian descent, to conduct the various duties. On some occasions I also took a back seat in the interviews with some of the senior administrators. The effect this had was sometimes amusing. For example, on one occasion a Head teacher looked enquiringly at me as my colleague mounted the podium to address the whole school assembly, as if to check that she had my permission. On more than one occasion in interview situations, the Japanese translator delivered an answer to me although I had not asked the question.

Challenging the expectations of our host provided us with further insight into aspects of Japanese culture. It was not our intention to be rude. The challenges highlighted issues of gender, age and ethnicity, which were able to discuss with our Japanese research colleagues. Moreover, both the expectations which our Japanese hosts held of us and the challenges to them offer insight into the research culture within which the cross-cultural case study was conducted. The notion of a hierarchy and a leader is clearly important in Japanese research, especially in an institutional context. Our experiences in respect of this taught us more about the conduct of cross-cultural case study whilst at the same time contributing to our database.

CONCLUSIONS

This chapter has attempted to engage with theoretical and empirical aspects of cross-cultural case study research. The definitions offered and the field-based examples drawn upon, have served to illustrate both the complexity of the approach and its capacity to yield rich, detailed and multi-faceted data. At the same time, they have attempted to confront some of the difficulties, contradictions and dilemmas inherent in the conduct of cross-cultural case study.

The approach taken in the study recounted here has demonstrated the capacity of cross-cultural case study to engage with definitions of culture, which exist at different levels within society and within institutional settings. Similarly, it has illustrated the way in which the notion of the Case can be developed to operationalize Glaser & Strauss' (1967) idea of progressive focusing. In doing this it has been possible to locate the Case not only in its immediate institutional context but also in a wider structural context. It might be argued, therefore, that cross-cultural case study has the capacity to provide analysis of social phenomena which, to some extent, bridges the structure-agency divide, which has at times proved divisive within social research (Bryman, 1988). The analysis of educational change in England and Japan (Yonokawa, 1996) has attempted not only to provide an analysis of specific aspects of the school curriculum in both countries, but also to locate this change in terms of historical and contemporary culture. The capacity for cross-cultural case study to achieve such different levels of analysis is dependent on a robust yet flexible research design. The example discussed in this chapter has attempted to offer an honest account of the challenges which research of this nature entails. In doing this, it has also been the intention to emphasize that cross-cultural case study is more than a method of data collection. It has the capacity to facilitate theoretically informed analyses, which whilst focusing on detail have a wider resonance which engages with definitions of culture on a number of levels.

NOTES

1. In addition to providing access to a variety of different aspects of the curriculum, the three subjects were also selected as the ones most likely to lend themselves to study by the English researchers who did not speak Japanese. It was assumed that the researchers would be able to understand some of what occurred in the English Language lessons and that Science and P. E. lessons would include activities which could be observed.

2. For a more detailed analysis of the impact of the changes in England and Japan see Chawla (1996), Morrison (1996) and Pole (1996).]
3. Although all interviews were tape recorded, to check on the accuracy of the translation, the tapes would need to be re-translated by a third party. This would have proved expensive and time consuming.

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EDUCATING EATERS: CROSS-CULTURAL APPROACHES TO FOOD AND EATING IN ENGLISH AND DANISH SCHOOLS

Marlene Morrison and Jette Benn

Editors' Commentary: *In a chapter which draws on research conducted in England and Denmark, Morrison and Benn discuss their different approaches to the study of food and eating in schools. The chapter focuses on the complementary and distinctive features of the authors' approach to their research, beginning with an account of the political and educational contexts in which the projects were located. Whilst both the English and the Danish studies straddled the divide between basic and applied research, they could be located within different disciplinary and intellectual traditions. Morrison approached the study of food and eating in schools from a sociological perspective. The school provided the setting to explore understandings among children and adults about the role and significance of food and eating not only in school but also at home in the context of children and young people's lives more generally. For Benn, the starting point was health education and her principal concerns are with pedagogy and curriculum as the routes to effect educational (and social) change.*

OPPORTUNITIES FOR COLLABORATION

Food, food use and eating have become the focus of growing national and international interest that has been fuelled by the global problem of endemic

hunger among a significant proportion of the world's population and by the paradoxical problems of 'healthy' and 'unhealthy' eating that derive from conditions of affluence and relative poverty among citizens of developed nations. Attention to food has been fragmented nationally and internationally, and academic disciplines have applied a range of theoretical and methodological frameworks to the study of food (Fine, Heasman & Wright, 1996). International conferences, focused upon food and eating, are indicators of the extent to which food has become a major issue for policy makers. For the authors of this chapter, a conference in Potsdam, Germany in 1993 was the venue for their first informal meeting and the basis for the networking that followed. These included visits between Morrison, based then at the Centre for Educational Development Appraisal and Research (CEDAR) at the University of Warwick, and Benn at the Department of Home Economics at the Royal Danish School of Educational Studies, Copenhagen. Subsequently, Morrison presented a paper at the Royal Danish School and the authors then gave a joint presentation in England as part of the ESRC Field Studies Seminar series on cross-cultural studies in December, 1995.

This chapter focuses upon the complementary as well as distinctive features of the authors' approaches to their work. It begins with a summary of the political and educational contexts in which the authors' projects were located.

POLITICAL AND EDUCATIONAL CONTEXTS

The time frame for collaboration was significant. Amidst increasing political disquiet about the neglect of preventative approaches to health and nutrition education, a growing public mistrust of 'experts' was also discernible in England and Denmark, fuelled partly by scientific pronouncements that appeared less than unitary or unanimous. With some scientific discourses about food being almost 'unintelligible' to those outside the scientific habitus, education and social science researchers in England and Denmark began to focus upon the multiple ways in which children and adults were responding to educational and information initiatives about health, food and eating.

Integral to increasing government concerns to improve the quality of British and Danish school children's diets, there was a commensurate belief in the ability of education to affect changes in both attitudes and behaviour towards healthy eating, linked to individual life styles. At the time of the English study (see below), connections between the concept of food choice, the practices of eating, and the role of schooling, were politically reliant upon an ideology of individualism to underpin strategies "encouraging individual responsibility, awareness and informed decision-making" (NCC, 1990: 7). Subsequently,

there would be emerging interest in the concept of 'healthy schools' (DfEE, 1999), a broad ranging term to include formal and informal approaches to the food curriculum and eating practices, and with the potential for incorporation into Personal Social and Health Education (PSHE) and the new Citizenship curriculum (albeit in the context of a food technology curriculum that has struggled to sustain its role (Morrison, 1996a)).

In Denmark, Benn noted a discernible shift towards viewing school-based eating as an educational opportunity to encourage healthy eating. Simultaneously, the focus of food education, still centred upon Home Economics (HE), was tending to move from the application of nutritional science in HE classrooms, to wider interests in food and eating. This was beginning to incorporate the social and experiential perspectives of pupils, and include a developing curriculum interest in the gastronomic and aesthetic aspects of food. Common to the English and Danish studies, was a joint interest in the micro-level of school experience, and in the juxtaposition between the curriculum needs of individual learners and the institutionalisation of eating on a daily basis in English and Danish schools.

CONCEPTUAL FRAMEWORKS: SIMILARITIES AND DIFFERENCES

Both studies straddle a continuum between what the Economic and Social Research Council (ESRC) has described respectively as orientated basic research (with aims to produce a broad base of knowledge which has some orientation towards practical applications which may have economic and social benefit) and applied research (investigation undertaken to gain new knowledge and with practical aims and objectives) (see also Murcott, 1999: 22). However, some aspects of each author's starting points were distinct. Benn, supported by assistance from the Danish Health Fund, conducted work as a lone researcher pursuing a Ph.D. As a teacher educator in HE and as a previous teacher of the same, her interests were initially subject framed, with primary concerns in pedagogy and curriculum as pathways to effect educational (and, in turn, social) change (Benn, 1996a, b). In a project directed by Burgess (Burgess & Morrison, 1995), Morrison's work was located as a project within a large scale programme funded by the ESRC – The Nations' Diet: The Social Science of Food Choice. Her conceptual framework was sociological, schools providing investigative settings to explore understandings among children and adults about the role of food and eating in schools and at the home/school interface.

Whilst these distinctions are important, they should not be exaggerated. For cross-cultural collaboration to occur, the authors recognised their work as part

of a collection of predominantly qualitative perspectives on food and education, 'collection' being, perhaps, an appropriate term in as eclectic a research arena as educational studies.

   Both authors shared a joint interest in the formal and informal ways in which children and adults made sense of food-focused education. The title of Benn's dissertation appears to lose important aspects of complementarity in translation, since 'kost i skolen' means more than 'nutrition in school'; instead, it encompasses the ways in which perspectives upon food and eating were understood and experienced in relation to time, space, and the social environment. Nonetheless, Benn's primary interests were in HE and with HE teachers and students. Her assumptions and hypotheses were that:

- Food in schools had been used and abused in different ways.
- Teachers held stereotypical views about HE education that had been constructed from tradition and longstanding routine.
- Practical issues dominated.
- The teacher 'knows what is best for the pupils'.
- Pupils liked practical subjects but had difficulty in understanding the 'theoretical parts' and were not involved in decisions concerning curriculum content.

Her aims, then, were to examine nutrition education in Danish schools, from the perspectives of the informal – the school lunch break – and formal curriculum practices. This is reflected in the thesis title: Circumstances, opportunities and prospects for nutrition education in the Danish 'Folkeskole'.

   For Morrison, schools were settings which, in common with other institutional sites like offices, factories, prisons and so on, provided investigative sites for observing that "food is not just something we eat" (Murcott, 1999: 14–15), and then to ask "why do children and adults eat what they do in school?" and "what do they learn about food and eating in school and in which contexts (subject lessons, the playground, on the way to school, and/or in the tuck shop)?" Underpinning both questions was a concept that dominated the wider programme, namely food choice. Morrison's interpretation of choice had strong sociological overtones, being fundamentally embedded in the social and cultural contexts of schooling and the extent to which children, in particular, might enjoy autonomy of choice, and in which circumstances. Interests in power and control were linked to practices of inclusion and exclusion in school (Burgess & Morrison, 1999). Thus, unlike the Danish study, her focus, and indeed previous experience, was not focused on a longstanding academic interest in nutrition education (or in HE) per se.

Moreover, explicit in the processes and findings of the English study was the recognition that, despite what nutrition educators might expect or wish to occur in schools, many of the forces that affected food and eating in schools had relatively little to do with nutrition or health education. Food choice, therefore, provided the key analytical mould, and as Murcott (1999) has remarked in relation to the larger programme in which the project was embedded:

> The [last] point might be obvious once stated. But it deserves constant reiteration and requires regular research confirmation. This way, at the very least, it will support those whose craft, profession, or commercial interest leads them to focus on food, on nutrition and health, on eating itself, who must strive to bear in mind that not everyone shares their preoccupations (p. 21).

METHODOLOGICAL FRAMEWORKS

In the English and Danish studies there was a preference for qualitative approaches to food and eating. Benn engaged in a combination of quantitative and qualitative approaches; Morrison eschewed combination in favour of an ethnographic study focused upon four schools. For Benn, an initial questionnaire survey to schools and education authorities provided a platform to establish patterns and regularities in nutrition education from which she developed a more dynamic account, through the use of qualitative interviews and observations with teachers and pupils in five case study schools. The methodological approach taken by Morrison shares with other ethnographic approaches the following features: first, an exploration of behaviour in every day settings; second, the gathering of data from a range of sources; third, a focus upon a small number of settings; and fourth, a concern with the meanings and human action, explored through verbal description, explanation, and observation.

In both studies there was a privileging of pupils' and teachers' understandings about food and nutrition over top-down assertions about what children and adults ought to eat and/or learn about food. In the Danish study the lens was much more narrowly focused upon HE teachers and upon HE pupils than in the English study, where more attention was also paid to naturalistic observation. Both studies produced knowledge to be assessed in terms of validity, reliability and public relevance, and sought plausible and credible explanations to the question "what is going on here?" Benn, in particular, drew inspiration from the phenomenological approaches of Kvale (1979, 1987, 1988, 1989). Morrison favoured an approach which has been described as "subtle realism" (Hammersley, 1990): firstly, the argument that ethnographies of food and eating are not scientific because they cannot be replicated was partially rejected because it rests, as Hammersley asserts, upon a false conception of the

role of replication in the natural sciences; secondly, whilst it is possible to argue that no knowledge is absolutely certain, knowledge claims were assessed reasonably accurately on the basis of their likely truthfulness. Common to both studies then, is the suggestion that there were three steps in judging the truthfulness of the findings: their plausibility, credibility, and communal assessment by the scientific and educational research communities of which the authors formed part.

Benn charted the research process from questionnaire design, through case study, to theory building, and what translated is described as 'perspectivation', by means of Table 1:

Table 1.   Research Process and Problems (Danish Study)

Research Process	Problems
1. Researching the field through questionnaires	1. Too narrow perspectives in the field or too broad? The questions too simplified or too difficult to answer
2. Selection of the questionnaires.	2. Misunderstanding of the replies, overinterpretation of results?
3. Election of teachers, pupils, schools for observations and interviews.	3. Too close to the interviewer?
4. Preparation of question guidelines for pupils and teachers.	4. Too guiding or too loose?
5. Interviewing.	5. The situation itself, the atmosphere (time, place, empathy, understanding), language, body language and formulation of questions.
6. Transcription of tapes.	6. Is the total situation brought to expressed? Is it possible for the language to cover meanings?
7. Listening and reading.	7. Preconceptions, interpretations- final conclusion?
8. Preparation of transcripts in relation to fields of interest.	8. Narrow or broad conception of context and contents?
9. Relation between interviews and single aspects or problems	9. Atomising in relation to the whole?
10. Theory-building – critical and concrete area for use.	10. Can the qualitative material be a basis of theorising?
11. Perspective- derived problematic – new research.	11. Same problems and angles in another disguise?

THE CASE STUDIES

In both the English and Danish research, the case studies were key elements. Each study gave detailed attention to the selection of sites. Benn selected case study schools on the basis of geographical and social differences. Unlike Morrison, the teachers selected for interview were known to her beforehand in her role as teacher educator; sensitivity to the issues of teacher professionalism and being seen in a teacher trainer role needed to be balanced alongside the advantage of some prior knowledge about the interviewees, with whom Benn engaged in successive interviews.

The English study was concerned with the processes of teaching and learning about food and eating as they were experienced and understood by children and adults in four comparative and exploratory case studies. The intention was to make multiple cultural phenomena investigable, whilst at the same time 'make' strange some of the very familiar practices of daily eating. Among the settings examined were the formal lessons in which food was discussed, as well as a range of sub-sites such as the school 'tuck shop', the school dining hall, the after school club, and, in turn, a number of informal settings: chewing sweets in a lesson, eating as pupils moved round school, and eating in the playground and at breaks and lunch times. Among the key questions addressed were: in what location did food use happen as teaching activity and as consumption, and where was it discussed? In what ways did the social context of the school, that is, whether it was urban or rural, working class or middle class, multi-ethnic and so on, influence the kind of discussion that occurred and the kinds of food that were eaten? In the English study, two schools were selected from the primary sector and two from the secondary sector, and one term was spent by Morrison in each school. Those selected allowed comparative analysis to be made, for example, on the age of children and attitudes to food and food choice. Schools were also selected from locations with contrasting rural/urban, socio-economic and cultural dimensions.

Research in the English and Danish schools illustrated the ways in which food and eating had specific significance, both in terms of collective eating rituals, as the focus and content of the formal curriculum, and in relation to various interpretations of food as official and unofficial knowledge, and as eating practice. Central to the research task was the importance of making connections not only with the biographies of teachers and learners (which in Morrison's case took her beyond the school and into the home) but also the organisational contexts in which teaching and learning took place.

If qualitative explorations of food and food use 'trade off' breadth for depth (Murcott, 1993), the studies illustrated the value of 'thick description' in making complex phenomena investigable. In the English and Danish case studies, attention was given to multi-site studies where, what Guba and Lincoln (1982) describe as the 'fittingness' of the case studies, was accompanied by a concern to investigate heterogeneous rather than homogeneous sites.

Table 2.   Schools in the Danish Study

Schools	Quantitative information			Situation	Characteristics
	total number of pupils	forms at each level	number of teachers		
1	500	2–3	47	Public school in Copenhagen in an area with flats from, 1960's. These make up a special quarter in the town.	The school is established in the same period as the surrounding apartment buildings. It is in one level. A social deprived area with many single parent families and immigrants.
					Elderly teachers with several years of engagement at the School projects of different kinds, mainly initiated by single teachers.
2	550	2–3	54	Public school in Northzealand in a residential area togehter with an old fisher town and summer cottages.	The school established at the start of the century but extended in 1970. Teachers of middle age. Pupils from the middle class, many parants self-employed and higher officials.
3	400	2–3	35	Public school in Copenhagen in an area with both appartments buildings and a residential area.	The school is 50 years, built in 2–3 levels. The home economics room is worn out. The teachers are of middle age with many years of engagement. Some developmental projects at the school.
					Pupils from a mixture of social classes and some immigrants.

Table 2. Continued.

Schools	Quantitative information			Situation	Characteristics
	total number of pupils	forms at each level	number of teachers		
4	400	2	33	Old private school in the central Copenhagen.	The school has small old classrooms, but a new decorated home economics room. The pupils come from many different areas in and outside Copenhagen. Many pupils from academic and wealthy families.
					School projects of different kinds.
5	300	2–3	56	Central, poor area of Copenhagen appartment buildings.	The school old but in good repair. The home economics room renovated. The teachers mixed in age.
					The school is in an area with many immigrants and socially poor families.

THE RESEARCHERS

Prior to examining the studies' findings, it is useful to reiterate briefly on the opening positions of both researchers, since these were reflected in the framing of the research questions, the kinds of data collected, and the analytical frameworks in which the findings were embedded. As a nutrition education expert, Benn was well known as a HE educator in Denmark; this enabled participants and researcher to speak a similar 'nutritional' language, and may have included implicit assumptions about the super and sub-ordinate positions of, for example, the interviewer and interviewees. For Morrison, presumptions about her background and the rationale for the research led to research situations that were both comforting and discomforting, but, together, added weight to overall understandings about perspectives upon food and eating in school. Where her role was understood as sociologist/researcher, the tendency

Table 3.   Case Study Schools in the English Study

SCHOOL 1	SCHOOL 2
Primary	Primary
Brook Street	Fieldgate
Numbers On Roll: 300	Numbers On Roll: 350
Co-educational	Co-educational
Urban	Rural/suburban
Multi-ethnic	Ethnicity: white
Recognised Socio-economic	Population
Deprivation	Area of Full Employment

SCHOOL 3	SCHOOL 4
Secondary	Secondary
Hallcommon	Hillgate
Numbers On Roll: 950	Numbers On Roll: 900
Co-educational	Co-educational
Urban	Rural
Multi-ethnic	Ethnicity: white
Mixed Socio-economic Catchment	Area of Full Employment, Unemployment
	Narrowly Focused

was for respondents to concentrate on the 'what is' rather than the 'what ought to be' of food-focused education. Where this was not understood (despite careful explication), responses tended to be drawn on a continuum between 'defensiveness' about the contribution of food-focused activities to school life and/or pupils' learning, and those linked to the researcher being cast as a 'champion' of health education or as a positive contributor to the revitalisation of home economics/food technology among subject teachers more concerned about their roles in school (and therefore more akin to Benn's position). In extreme, this presented Morrison with interesting interactive situations, as in the case of the interviewee who began the interview by opening a bag of fish and chips in a classroom at lunch time with the opening comment "I suppose this shocks you but . . ." (the interview proceeded between mouthfuls) or in the case of the staff room report back session when a primary school teacher railed at "people like you who think they can tell us what we should be eating. My daughter has chips every day and they haven't done her any harm." Moreover, unlike the Danish researcher, Morrison moved beyond the physical spaces of HE, and substantive concerns about nutrition education, to consider the home-

school interface of food use, specifically in relation to interviews with parents whose children had completed food diaries (see below).

FINDINGS

Research findings illustrate complementary as well as distinctive aspects of the English and Danish studies. These are itemised under four main headings, not all of which are given equivalent attention in both studies, for the reasons already stated. The headings are: curriculum issues; school policies and practice; institutionalised eating; and the social construction of food and eating.

Curriculum Issues

Researching food and nutrition makes possible a range of intellectual connections. In the Danish study, the importance and necessity of learning about food and nutrition were assumed rather than a topic for investigation. The critical issues related to exploring HE teacher's understandings and beliefs about HE and implications for classroom practice, and also upon pupils' understandings about classroom practice, linked to broader understandings about food and eating (see later sections). In the English study, findings were drawn from evidence that illustrated the continuing marginalisation, com- partmentalisation, and fragmentation of food-focused knowledge by staff and pupils. In a Durkheimian sense, food was seen as part of the 'sacred' elements of school knowledge which was framed "in the context of knowledge transmission" (Atkinson, 1985: 135) and was subject focused. This was distinct from the more 'profane' aspects of non-statused activities like lunch-time eating or the ill-defined status given to health education. This offered few opportunities for assessment and examination, and by pupils, was considered largely irrelevant to their careers beyond school.

Among tensions in the English primary schools were those linked to an 'overcrowded' curriculum and to increased demands for subject rather than generalist teaching. In the school where food was discussed outside the 'core' as extra activity, the continuation of interest was most at risk, and more dependent upon the individual enthusiasm of specific teachers. As mediators of the curriculum, teachers' behaviour was reflected in the extent to which food issues were prioritised or minimised as 'sensitive' on multicultural or socio- economic grounds, or viewed as a parental rather than a teacher responsibility.

Secondary school responses to curriculum initiatives were similarly linked to specific schools' histories, to subject and department cultures, and to the

relative negotiating strengths of different departments. Despite the holistic
tendencies of vocational initiatives, evidence of tensions between subject and
non-subject based departments had been accompanied by a gradual demise of
HE, its displacement as 'food technology', and the insecure position of food
and nutrition within health education programmes. Where HE had become part
of technology faculties, this did not mean an in-depth understanding of the
curriculum content of technology or of HE/food technology by either party.

In relation to food, there was a hierarchical gradation of knowledge,
understanding and skills: Advanced levels in science, General Certificates of
Education (GCSEs) in HE, technology, science, economics, history, and
geography, and key attainment targets for the core and foundation curriculum
subjects. In addition, there were cross-curricular approaches to health
education and a growing emphasis upon active tutorial work and Personal
Social and Health Education (PSHE) in which food, nutrition, and lifestyles
education was becoming increasingly wedged between more highly politically
profiled sex and drugs education programmes.

Contradictions were apparent. An emphasis within cross-curriculum guide-
lines upon self-awareness and responsibility, designed to empower individuals
to make healthy food choices, for example, had been, for the most part, devoid
of consideration of the "social, economic and political limitations upon
individuals" decision making (Lawson, 1994: 8). For several teachers, there
was a lack of awareness and/or scepticism about food 'advice', which was
accompanied by concerns that food as a 'sensitive' issue could invoke negative
judgements about students' family lifestyles. In art classes observed during the
project, students continued to design new boxes for burgers, whilst being
instructed to question the links between burger production and land use in
geography lessons, and partake of a daily menu of burgers in the school
cafeteria.

Generally, the formal curriculum for food and nutrition in the English
schools studied was circumscribed by the subjects and time allocated for them,
and proportionately to assessment potential. In this sense, core subjects were
allocated more time than cross-curricular themes. Status distinctions were
maintained between food 'knowledge' and food handling and preparation,
which, in turn, had pedagogical implications. Cross-curricular themes occupied
the grey areas between them. This was not to deny that different subjects
offered positive reinforcement as well as a duplication of learning. Yet the line
between duplication of effort and the marginalisation of food and eating
remained a fine one.

Findings from the English study highlighted the need for greater curriculum
coherence in policy and practice, and for a re-assessment not only of the ways

in which food knowledge was framed but also upon its interpretation by teachers and learners. As will be shown, this was also among the conclusions of the Danish study, but not within an analytical framework of subject marginalisation.

Whole School Approaches to Food and Nutrition

Both the Danish and English studies focused on the formal and informal ways in which children and adults learned about food and nutrition in schools, including lunch time provision. Each study drew, in part, upon historical frameworks for analysis. Not surprising, given the emphasis upon HE in the Danish study, more historical emphasis was attached to the subject's development. In the national contexts for both studies there were needs to re-assess emerging political, nutritional, and educational interests. As Danish schools moved towards considering the educational opportunities provided by school lunch breaks, at the time of the study the tendency was for English schools to be moving in the opposite direction. In England, Compulsory Competitive Tendering (CCT) and the Local Management of Schools (LMS) were continuing to break down traditional patterns of school catering in which nutritional standards had already been diluted as a result of the 1980 Education Act. Here, the process of disconnecting the nutritional and welfare issues of school eating from its educational significance, including links to teaching and learning about food and nutrition in school, made the prospect of coherence between the formal and informal curriculum both challenging and contentious. Pupils' eating was becoming increasingly a private matter, yet subject to discipline, containment and control, as well as financial resources on school premises.

None of the four English case studies had written policies in place for food and nutrition and there were mixed views about whether this was desirable. Moreover, changes in teachers' conditions of service had accelerated a decline in viewing food and eating as educational inclusive. In their final report, Burgess & Morrison (1995) pointed to a range of positive outcomes when eating was given educational, health promotional and/or social status by school managers. During, 1993–1994, when other priorities were deemed more pressing, challenges remained formidable.

Institutionalised Eating

Given the specific focus of the English study, it is not surprising that more attention was given to institutionalised eating. Evidence from the case studies

revealed school-based eating which punctuated the daily experiences of pupils as day long as well as lunchtime activity. Arrangements at lunchtime focused upon the insecure position of ancillary workers – dinner ladies – as temporary exercisers of power and influence. Illustrated in the research were the ways in which the organisational arrangements for eating influenced the processes of pupil and pupil/teacher relations, including the configuration of eating groups and their social identity, as well as providing contexts in which pupils found scope for contestation, conflict, and autonomy. Whether intentional or not, food could be used in a divisive way to create social divisions in terms of marking out particular attributes. What schools had in common was the propensity for food, whether available or scarce, uniform or varied, punishment or reward, to become part of a struggle for control. In this respect the school lunch and the school dining areas were not only social events but also occasions when a vast array of behaviour was played out in terms of organisation, ritual, division, and control (Burgess & Morrison, 1999). Institutionalised snacking and fast eating was, in the English secondary school case studies, an organisational strategy and institutionalised necessity that contributed to a culture of widespread eating throughout the day and in a range of sub-sites. From a sociological perspective, the data challenged the myths of school consumption choices as 'free' 'independent' or 'non-political'. It also provided further insights into the mechanisms of social division and control in schools as they were experienced by staff and pupils. School snack counters, tuck shops, and after-school clubs disguised status differences and distinction among pupils and between schools, and made food consumption appear homogeneous and consensual. Yet the apparent freedom to purchase a selection of items made it more difficult to recognise inequalities in eating among young people.

Social Constructions of Food and Eating

A key aspect of both studies was the interrelations between a range of meanings about food-focused education. Whilst, in the Danish study, the emphasis was upon pupils in HE classes and upon HE teachers, the English study looked at the interconnections between the perspectives provided by teachers (not necessarily HE teachers), pupils, dinner ladies, and parents.

Pupils

In the Danish study the categorisation of pupil data, based upon interviews, was grounded in the development of at least two conceptual schema. In the first,

Table 4. Pupils Concepts of What They Learn, In Relation to the Practical and Theoretical Elements of HE

practical elements		theoretical elements	
cooking	other practical work	general aspects	special aspects
Learn to	*Learn to*	*Learn that*	*Learn about*
- cook - bake - make dishes - make recipes - how many foods necessary to cook - make pancakes and potatoes - make bread, cakes and fish make the small dishes as buns and beefs - warm dishes	- do the dishes or use the dish washing-machine - to clean - to handle kitchen tools - to use tools	- it is not healthy to eat some stuffs - what is healthy - how to become healthier - to make propositions for breakfast, lunch and dinner	- vitamins - study what is inside the fishes

Benn analysed pupils' perspectives upon what they considered they learnt in HE, using a demarcation between 'learning to', 'learning that' and 'learning about'.

'Learn to' dealt with the practical aspects of the subject as the pupils perceived them. 'Learn that' included pupils' understandings about the degrees to which some foods were thought to be 'healthy' or not – and raised the issue of choice, linked to what pupils felt they 'ought to eat'. 'Learn about' related much more closely to the background knowledge that underpinned the study of HE – nutritional science – like 'vitamins' and 'what is inside the dishes'. Pupil data illustrate the extent to which nutrition education, as HE, was seen essentially as a practical subject ie learning how to cook. Building upon such data, Benn went on to explore the meanings pupils gave to food, when it was discussed in relation to making or buying food for oneself and for others, in Benn's case for the family. Part of the Danish study is, then, an exploration of the ways in which food and meals were discussed by pupils first, as 'ego-centred consumers' and secondly, as 'eco-centred consumers'. The transition is summarised in Figs. 1, 2, and 3.

In the English study, data analysis was divided along secondary/primary lines and included pupils who were not taking GCSE in Food Technology. As

Food and meals for oneself	
Choice	Reasons
1. Buy a burger.	1. It tastes good.
2. Something ready made.	2. . . .to put in the micro oven.
3. Buy something convenient, . . .	3. . . . then you do not have all that fuss.
4. It depends on how hungry I am and so on. **If you are very hungry?** Then I will buy some proper food. No it is only sometimes, I go to the burger shop.	4. Because sometimes I also think about it, then it will not help with some fast food. Then I rather prefer some decent food.
5. Buy something ready made – a bag of 'biksemad' (hash). **But was it only for you?** Then I think I would go to the burger shop once in a while.	5. . . . because if it is going to be that every day I will grow fat and swollen.

Fig. 1. Food and Meals for Oneself.

Food and meals for me and my family

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1. I would buy nearly the same as my mum. Meatballs, pizza and rice . . .

2. . . . try to make meat balls.

3. . . . try to make something we have learnt here in the school.

4. Now, I would take account of my mum and dad and they like Danish beef. so I would buy some minced meat, and so I would make some white sauce, not brown, white sauce is not as fattening as brown, it tastes good, also because it is healthy. And salad. The dressing we make ourselves. Garlic dressing or something.

5. I would buy something ready made, a portion of 'biksemad' – hash.

Why would you not go to the fast food store?

It is too expensive to buy for 5 persons.

Fig. 2. Food and Meals to Me and My Family.

Food and meals for others or in general

– I would take care that they got *clean food*, not chicken or something.

– They should not eat rye bread only. I would tell them not only to eat the spread on the bread and then give them some potatoes. I would give good stuff so they could live healthy. Vegetables and a little of everything.

– Give the children healthy things to eat. And they should drink milk with vitamins. I just do not want to get married, because I will live together with my mum and dad.

– I would take care that they did not get too fat at the same time. And that they did not have candies every day, but healthy food and not fries every day.

– They were allowed to decide themselves when they got a little older.

– It is not very clever if they (your children) only got 80 years because the die from a stroke or they are too fat or something.

Fig. 3. Food and Meals for Others.

in the Danish study, secondary pupils acknowledged the gaps between subject-focused 'nutritional' knowledge and the behaviours which they saw replicated by themselves and others in and beyond school. Preferring cafeteria style 'choice' to traditional 'school dinners', they made suggestions for improved school-based eating which included its organisational arrangements. Without exception, secondary school pupils interviewed prioritised home, family, and the media over school as major influences upon their food consumption. This was not to deny the factual input provided by school in terms of food 'knowledge', but data analysis showed an emphasis by pupils upon its segregation from their prioritisation of eating preferences. The secondary school data from the English schools also offered further detailed insights into young peoples' perspectives upon vegetarianism, and upon gendered, and class-based eating patterns, and eating disorders (Morrison, 1995). Together they illustrate ways in which food and drink were part of props used by young people to develop and stage their self-image (Goffman, 1959). Evidence from the primary school data suggested that food choices were much more dependent upon adult influences, particularly parents, although such relationships were complex and mediated through income, culture, and the media.

Teachers

In the English study, the data from teacher interviews replicated a range of adult perceptions about food and eating (Lannon, 1986) and a stress upon 'balance', 'moderation', and 'common sense' (see also Haastrup, 1993). Teachers were sceptical of food experts at the same time as they recognised gaps in their own 'nutritional' knowledge and behaviours. Teachers' commitment to food-focused education spanned a wide spectrum, from those who considered food-focused education to be the primary responsibility of parents, to those who considered it to be a key component either of their subject's focus or of life skills, in which teachers needed to be central and collaborative actors. Not surprisingly HE teachers were at the latter end of the spectrum. For the home economists/food technologists interviewed, food-focused education was also part of an ongoing struggle to retain resources, including time. Secondary school teachers interviewed transmitted food knowledge in ways that showed minimum recognition of the links with other subject areas. Not all teachers recognised or acknowledged the gap between the rhetoric of 'healthy eating' that they, for the most part espoused, and the reality of school-based eating.

Benn's study of Danish HE teachers only, comprised a series of life history interviews with five HE teachers over a period of months. Her analysis applied a series of ideal types (Benn, 1996b) – the householder, the craftswoman, the

THE HOUSEHOLDER

THE GASTRONOME   THE CRAFTS(WO)MAN

THE HOUSEWIFE
(MOTHER)

IDEAL TYPES/TEACHER ROLES
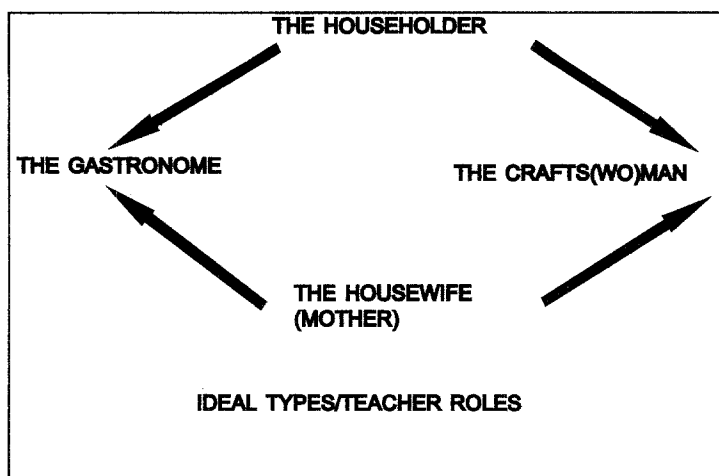
(BENN 1996)

Fig. 4. Ideal Type or Roles of the Home Economics Teacher (Benn, 1996).

housewife, and the gastronome – to consider the key influences upon the teachers she studied, and linked such understandings, in turn, to their teaching strategies. Ideal types were discussed in relation to the historical development of the HE curriculum, for example, during periods when the 'craft' of food preparation and cooking was first enhanced, and then reduced as the role of women in the home and in employment changed. Most recently, as Benn notes, increased tendencies towards 'eating out' and the internationalisation of food and eating, has been accompanied in Denmark by a greater curriculum emphasis upon gastronomy, aesthetics, and taste (in interesting contrast to the emphasis on food in England as a life skill rather than as an artistic or life-style enhancing curriculum activity).

The Home/School Interface

Pupils' Diaries
Only the English study pays attention to the role of 'significant others' who might influence teaching and learning about food and eating in school and at the home/school interface. This includes attention given to the role of 'dinner ladies' in school (most of whom were also parents) and the role of parents.

Interviews were the main research tools in relation to parents; for pupils, Morrison used interviews and diaries. The latter were used to provide a log of eating activities: to encourage as far as possible a free-flowing account of food-related events. The intention was to use qualitative strategies rather than food intake measures to record the subjective experiences of children and young people. In relation to pupils of mixed ability and different writing skills, and in the absence of assessment 'rewards' for completion, the data analysis was both age-related and challenging. Examples from the primary school diaries, shown as Figs. 5 and 6 illustrate this.

As Burgess notes, the three example diaries illustrate different styles in keeping a diary of food and eating, and the instructions were interpreted in different ways. Of methodological interest is the extent to which comparison could be made between different diaries, a significant issue when wishing to establish common themes and engage in concept-building. Overall, the contribution of diaries, when used in conjunction with interviews and observations, was to provide an alternative genre for prioritising 'the children's voice' in social scientific research.

Dinner ladies
In the English case studies, the adult role models available to pupils at lunch time were dinner ladies, predominantly drawn from the areas in which the schools were located, and often from the school class group from which the children was placed. (This was not a central issue in the Danish research, where lunch time supervision was still much more of a teacher than ancillary staff responsibility.) Dinner ladies' interpretations of eating, 'eating up' and food choice, and of lunches as social or asocial occasions were, albeit temporarily, powerful influences (Burgess & Morrison, 1999, Morrison, 1996b). At secondary level, internal policing of eating space and eating queues were the dominant preoccupations of dinner ladies; this included the reporting of disruptive behaviour.

Parents
Observations in English schools illustrated the ways in which parents were able to play positive roles in food-focused education either as classroom assistants or as 'dinner ladies'. In interviews (usually at home), parents more than teachers were likely to view teaching and learning about food and nutrition as a shared responsibility; this was seen by parents in terms of teachers providing knowledge about food, and parents inculcating the skills of eating. Among the

socio-economically secure, there was an intense interest in 'healthy eating' as an issue of parental concern. Among the less secure, this was also articulated, but more in terms of concrete behaviours like parents preparing 'proper' meals and children brushing their teeth, and, as with some teachers, there were

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Dear [Name of pupil]

A FOOD AND DRINK DIARY

I hope you will help me. I am a researcher who would like to learn more about what children eat and drink. If you write in this diary, it will help me to know more.

So, next MONDAY, TUESDAY, WEDNESDAY, THURSDAY, FRIDAY, SATURDAY, and SUNDAY, please write in your diary. It may help you to write if you think about answering these questions:

WHAT DID YOU EAT AND DRINK TODAY?

WHEN DID YOU EAT AND DRINK TODAY?

WHERE DID YOU EAT AND DRINK TODAY? (at SCHOOL, HOME, SOMEWHERE ELSE?)

DID YOU EAT ALONE or WITH YOUR FAMILY or WITH YOUR FRIENDS?

DID YOU GO ON A VISIT and EAT THERE?

DID YOU ENJOY A CELEBRATION? (like a BIRTHDAY, ANNIVERSARY)

DID YOU LIKE WHAT YOU ATE AND DRANK?

PLEASE WRITE ABOUT HOW YOU FELT.

Thank you very much for your help. Please return the diary to school after seven days.

Yours sincerely

MARLENE MORRISON
Research Fellow

(Burgess, 1994: 7)

Fig. 5. Food and Drink Diary.

Diary 1

> *Breakfast.* cornflakes, orange juice. I ate it at home by myself.
>
> *Lunch.* sandwiches with ham cheese lettuce crisp two chocolates and a drink. I ate at the Birmingham science museum with my friends.
>
> *Tea.* Cup of tea with toast cakes and biscuits. Ate by myself.
>
> *Supper.* Chips vegetables beefburgers fish fingers gravy. Ate with my family.

Diary 2

>   21/6/93
>
> Today at dinner time I ate sandwiches, a chocolate, crisps and I drank some coke. In the morning I drank some tea. When I came back from school I ate rice pudding, chappati, and curry. Then later some fruit. I ate in the morning in the afternoon and at night. I ate at Birmingham museum and at home. At dinner time I ate with my friends, and in the morning and night I ate with my family. I went on a visit and ate there. No I did not enjoy a celebration. Yes I did like what I ate and drank today. I felt very hungry today.

Diary 3

> *Monday 21th June, 1993*
>
> (1) Today I ate 2 Turkey Batches, crisps, a bottle of pop.
>
> (2) I ate and drank at dinner time.
>
> (3) I ate and drank somewhere else.
>
> (4) I ate with my friends.
>
> (5) Yes.
>
> (6) Yes.
>
> (7) I liked what I ate and drank.
>
> (8) I enjoyed eating and drinking.

(Burgess, 1994: 8–9)

Fig. 6. Examples of Completed Pupil Food and Drink Diaries.

parents who took the view that food and eating was and should remain peripheral to the central concerns of schooling.

SUMMARY AND CONCLUSIONS

What links the English and Danish studies and the collaborative discussions that have accompanied the work of both authors has been the need not only to focus critically upon the role of food-focused education, including eating, in schools, but also upon the application of mainly qualitative frameworks in order to understand and interpret the meanings and experiences of educational actors –mainly teachers and pupils, and in the English study, parents and dinner ladies. As issues of food and eating are increasingly prioritised by the food industry, by governments in the U.K. and Denmark, and by health professionals and educators in both countries, the studies illustrate the various ways in which food is self-evidently open to investigation by educationalists and social science researchers. Ultimately, what might be of most interest to readers has less to do with the specific (and focused) rationales taken by the authors and more with their specific interpretations of what qualitative research might include and/or exclude in their interpretations of the words and actions of others.

Since, 1994 the impetus for the burgeoning of qualitative inquiry into food and eating has been accompanied by concerns that such interests might transcend national boundaries, and be relevant for policy making and practitioner communities which extend beyond academia. Following Ward Schofield (1993), there appears to be at least three useful targets for generalising the data from the English and Danish studies: what is, what may be, and what could be (p. 221). Studying what is included choosing research sites and actors on the basis of multi-site case studies and typicality. Studying what may be reminds us of the research designs for each study and the intended 'fit' with likely future trends and issues in school-based food and eating in England and Denmark. Approaches to what could be suggest avenues in which policy-makers might be primarily interested: by drawing out, in particular, the extent to which existing practices might need to change in order to make future government aspirations operational. Qualitative inquiry is about engaging with the social scene, experiencing it, and seeking to understand and explain it; undertaken with skill, and including cross-cultural perspectives, such inquiry assists greatly in the understanding of the critical relations between social structures and actions. In the representations of schooling for these studies, it is concluded that where more is currently being asked of food-focused education, much more remains to be evaluated systematically.

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QUALITATIVE ANALYSIS: PHASES, TECHNIQUES AND COMPUTER USE

Fred Wester and Vincent Peters

Editors' Commentary: *The focus in this chapter switches to the process of data analysis. Wester & Peters provide an account of Kwalitan, a software package for the analysis of qualitative data, which has proved particularly useful in the context of cross-cultural research. The technical discussion of the software package is underpinned by theoretical concerns for a common conceptual framework, which could provide the basis for cross-cultural comparison of social phenomena. The authors draw on a grounded theory approach to data analysis as they invite readers to consider the application of Kwalitan to their own cross-cultural analyses.*

SOME GENERAL PRINCIPLES OF QUALITATIVE ANALYSIS

One of the major difficulties research has to deal with are the aims at describing and comparing phenomena across cultures and lack of a common conceptual framework that holds for all cultures under study. This implies that, before describing and comparing, one has to search for a common conceptual frame, i.e. one has to look for characteristic key concepts and formulate them in such a way that they can be used to describe and compare phenomena across cultures. This is where qualitative analysis comes in.

The aim of qualitative research is the systematic development of theory by thorough inspection of social reality. Starting with a roughly defined theoretical framework (often in the form of a set of sensitising concepts), the researcher continuously tries to confront this framework with the field under observation in order to specify, define and ground these concepts and to formulate a substantive social theory. There is still a growing amount of literature on qualitative research procedures (e.g. Denzin, 1978; Spradley, 1980; Burgess, 1982; Hammersley & Atkinson, 1983; Bryman & Burgess, 1994; Maso & Wester, 1996). One of the most elaborated methods of qualitative analysis is the Grounded Theory Approach (GTA). The GTA is cyclical in the sense that data collection, analysis and reflection on observation and analysis continuously alternate. Important principles are "theoretical sampling" and "constant comparison" (Glaser & Strauss, 1967; Glaser, 1978; Strauss & Corbin, 1990). When the researcher discovers important elements of the developing theory, he[1] collects new data in order to specify, confirm or correct these findings. Analysis in this view means a constant comparison of findings in different situations until no more new insights can be obtained to develop the grounded theory. In cross-cultural research this means comparisons of cases both within and between cultural groups to elaborate what is common to all and particular for each group.

We have developed our own variant combining GTA-principles with the insights from the increasing amount of literature on qualitative analysis published since the 1980s and with the use of the computer program Kwalitan as an administrative support tool (Wester, 1984, 1995; Peters & Wester, 1994).

In this chapter we present a stepwise procedure for qualitative analysis and specify how the computer can be used in this process.

Characteristics of Qualitative Analysis

Analysis is often equated with a more or less distinctive phase in the research process after the collection of the data, in which all kinds of operations on the data are executed. This is not the case in qualitative research, because here we find the study follows a complex course, where periods of reflection, observation and analysis alternate each other. Besides, the analysis process in the distinctive phases does not concern the same kind of operations and procedures. So in qualitative research, there is not something like one separate 'phase of analysis'. Moreover, the equation of analysis with operations on the data material gives a rather limited view of what analysing is all about. The work to be done to allow these kinds of operations is especially missed in this view of analysis.

This becomes clear if we examine the meaning of the term 'analysis'. According to the New Websters' dictionary (1991: 32) 'to analyse' refers to breaking down in constituent parts, to resolve in elements. Interpreted in this way, during the process of analysis the data material is unfolded into relevant parts or aspects. One may think of the different relevant aspects the researcher wants to take into account.

This interpretation of 'analysis' refers to the key role of the researcher's framework during the analysis. Concepts, indicators, variables, categories and classifications play an important part, because they allow the researcher to unfold and specify the research problem and, later, to unfold the collected data into components to be separated, defined, ordered and classified.

But there is more. The perspective of the researcher is helpful not only in unfolding the phenomenon under study. In most cases it offers a framework to relate concepts and variables to each other. The research objective in most cross cultural research is not just unfolding the phenomenon, but the composition of an overview of the patterns to be observed in the data to define differences and similarities between cultural groups.

This may clarify that analysis has everything to do with the (partially still unelaborated) analytic framework of the researcher. To a large extent 'analysis' takes place before operations on the collected data. In this view, formulating the (temporary) problem statement is an initial form of analysis with clear consequences for data collection and operations on the data, that are related to the framework of the researcher. And the preparation for the field work, in terms of, e.g. a topic list for interviewers, is a kind of analysis, because it explicates what one wants to know and to relate. Moreover, one might, as Bogdan & Biklen (1982) do, distinguish between analysis activities that take place during the fieldwork from analysis activities that are executed after the fieldwork. Although they refer to ethnographic research, this distinction may be applied to interview studies, if one sees every interview as an instance of fieldwork, or other kinds of qualitative research.

In the view of analysis proposed here, research activities are considered as a search for an answer to research questions. All steps that contribute to the formulation of an answer are, as such, analysis steps. In terms of Spradley (1979, 1980) it concerns a Question-Answer model to evaluate research activities, but here the 'questions' are not the questions asked in the interview, but the research questions related to the problem statement, and the 'answers' are not the answers of the respondents, but what the researcher formulates about the research questions by interpreting the collected data on the basis of his framework.

In qualitative research data usually consist of text-like material such as observation notes, conversation protocols, interview tapes, documents, etc. It is raw material in the sense that it must be processed in one way or the other in order to become suitable for analysis. The material must be stored in an organised way, easily accessible, easily manipulatable and it must remain possible to (re)organise the material as often as desired.

In qualitative research, the analytic elaboration of the researcher's perspective is strongly connected with the analysis activities. This has to do with the fact that in qualitative research, the researcher's perspective is often only roughly defined in advance, becoming more focused on the research field in question during the course of the study. Hence the analysis can not be seen as one separately defined phase in the research process as a whole. It is a recurring component in the cycle *reflection – observation – analysis – reflection,* through which the researcher must continually pass.

By viewing *analysis* as a recurrent phase in the cyclical research process, some characteristics of the analysis process become clear.

Firstly, analysis always arises from questions to which the material is subjected. These questions have emerged from the reflection phase and have acted as the driving factor behind the observations. In the collected material an answer to these questions is sought and the material is organised and processed accordingly.

Secondly, analysis does not all the time focus on the same questions. The cyclical nature of the research indicates a progression in the sense that, new, more specific questions can be asked on basis of the insights developed, which may then initiate a following cycle. The development of the questions is closely connected with the elaboration of the theoretical framework.

By means of *data collection* (the observation phase of the cycle) the researcher ensures access to the empirical material in which answers to the research questions must be sought. This can entail the collection of (new) material. In addition, however, it is also possible for a researcher (and this occurs primarily in the later stages of the analysis) to select specific parts of the material relevant to a certain phase of the research from the available material already analysed in part. Selecting interviews is inspired by theoretical considerations emerging from the previous analyses (this is referred to by the term 'theoretical sampling').

Supported by the observation and interpretation activities, the researcher attempts to derive the concepts, features, characteristics etc. relevant to the scope of the subject to be examined from the selected research material, or to further work out or substantiate concepts etc. which are already known. The main activities of the researcher consist of reading the selected material from

the viewpoint of certain questions he may have, e.g. interpreting the contribution of the particular respondent and/or comparing interview segment codes to find an answer. Dependent on the phase of the analysis, this process of reading, interpreting and comparing is controlled from within the theoretical framework in development.

Reflection: Working with Memo's

The results of this interpretation are processed via the reflection process. This process has two functions: on the one hand, the researcher may fill in the conceptual framework based on the findings, while on the other hand, reflecting on the material and on the outcomes of analysis may result in new research questions, which can be examined more closely in a next phase of the research. An important tool in this phase is the usage of memos.

Memos are a representation of the reflections made by the researcher as a consequence of the observation and analysis activities. They usually bear reference to (parts of) the analytical framework and to the research problem. Memos, in fact, steer the direction of the analysis and of the study as a whole.

Memos may fulfil a wide variety of functions in qualitative analysis. To derive the utmost benefit from these functions, four different types of memos are distinguished.

Concept Memos
The aim of concept memos is to record the meaning of all significant concepts applied during the course of the analysis. During the analysis, concept memos enable the researcher to deal logically with the meaning of the concepts employed and to record any changes which may occur in the meaning of these concepts. At the beginning of an analysis, we will include a description of the 'sensitising concepts' in the concept memos, while the descriptions of the codes developed during the first phase (the so called open coding) are added later. And during an even later stage of the analysis, the concept memos will contain the concrete forms of the codes, as expressed by indicators, variables and categories of variables. In addition to the description of the concept concerned, references to the segments in which this concept is clearly apparent (and from which citations can be taken for the research report) as well as references to other concepts to which the concept is related, can be included in concept memos.

Profile Memos
Profile memos aim to give a characterisation of the observation unit. For example, if in a cross cultural study members of different cultural groups are

interviewed, each respondent can be treated as a unit to be described in terms of the concepts employed in the analysis. Profile memos become particularly important when the focus of the analysis shifts away from segments of text, as in the first part of the analysis, towards cases. In fact, profile memos provide a kind of recap of the material of a respondent, in terms of the most central concepts. Even in the exploratory phase of analysis an overview memo of the interview can play a significant role during the analysis, as the coding of the segments must be carried out, as far as possible, from a comprehensive idea of the unit as a whole.

Theory Memos

Theory memos concern the analytical framework constructed before starting the research, which is elaborated during the analysis. In theory memos, the components of the analytical framework are described together with the connections between the components of this framework. Hence this involves the interrelationships between the 'sensitising concepts' and between these and the substantive or field related concepts developed later, as well as the relationships between the latter. Abstracts of literature studies may also be included in theory memos. Concepts, variables, categories and the like are described in concept memos while the relationships between them are elaborated in theory memos. There are, moreover, specific memos containing a diagrammed outline of the theory-of-the-moment.

Method Memos

Method memos concern the methodological decisions taken in the study. Typical subjects dealt with in method memos are, e.g., the topics to be addressed in the interviews (topic list), how to proceed in selecting new cases, the characteristics that are going to play a role in making this selection (c.f. the principle of 'theoretical sampling'), the questions focusing the analysis of the data, etc.

   Tables and overview memos may also be considered as method memos, but qualitative overviews such as these tend usually to be so large that a wall (on which the overview is displayed) is more likely to be reserved for them than a method memo! Of course, reminders can be written in order to keep up with all the overviews compiled.

The Process of Analysis

By viewing analysis as a recurrent phase in the cyclically developing research process, it becomes clear that qualitative analysis is an *iterative process* that is *tentative* in nature. The phase in the theory development in which the researcher has arrived determines the way in which the material is examined.

The procedure consists roughly of the steps that are depicted in Fig. 1.

This outline shows that after the data have been collected, the research material must first be converted into a readable form. Especially if these data should consist of taped interviews it is necessary to type these out. But even when the material is made up of existing texts, it is often necessary to first make copies to work with. The material is subsequently divided into text units or segments, constituting the units of analysis. The exact composition and size of these segments is left to the researcher. One possibility would be to have a new segment start with each main question of the interviewer. A general code is subsequently assigned to each segment in order to achieve a preliminary division of the material. This general code usually gives an indication of the content of a segment and can often be taken from the topic list of the interviewer. The activities which are undertaken to mould the research materials into a readable and manageable form are referred to as *transcription.*

> data collection

> presenting the texts in a readable and manageable form

> preliminary ordering of the material
    - diversions into segments
    - assignment of a first general code (topic) to the distinguished segments

    > selecting material

    > reading and interpreting a text

    > developing and assigning codes

    > incorporating the findings into the conceptual framework and formulating new research questions.

> further analysis
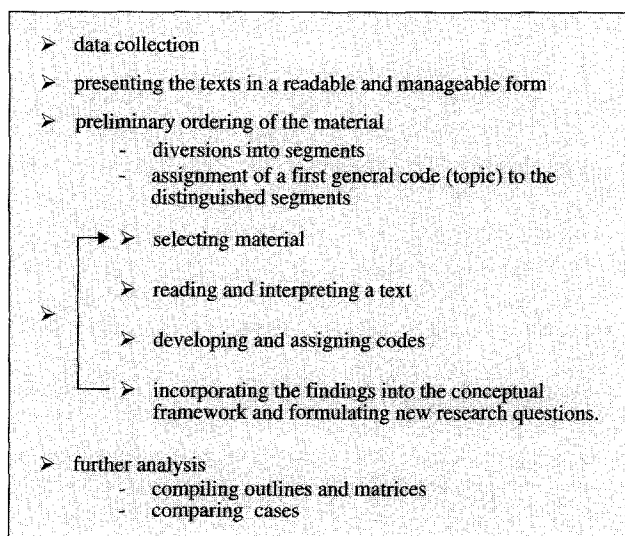   - compiling outlines and matrices
    - comparing cases

Fig. 1. A Rough Outline of Qualitative Analysis.

After these preliminary activities have been completed, the actual analysis can start. After optionally making a selection from the material, the process of observation (i.e. reading from a certain viewpoint), interpretation and depiction of the significance of the text can begin. The concepts and/or features found by the researcher in the segments are coupled to these segments as codes. After incorporating the findings into the conceptual framework and formulating new research questions, the process is repeated a number of times, until the researcher feels that the conceptual framework has been sufficiently developed and that the concepts, features and characteristics have been defined with sufficient accuracy. The researcher will then carry on additional analyses. Up to this moment text units (segments) are the main units of analysis, but from now on the focus will be on the case, e.g. the respondent. The researcher attempts to rise above the text of the interviews and to make statements at the respondent level. An important tool during this phase is making matrices (see for example Miles & Huberman, 1993) and the analysis is focussed on comparing cases.

In cross cultural research the exploratory process of ordering the material is done on material from each cultural group apart. These within group explorations yield a conceptual framework with general and group specific codes. In the further analyses cases from different cultural groups are compared to elaborate common and group specific dimensions.

QUALITATIVE ANALYSIS: PHASES AND TOOLS

The elaboration we present here is based on our experiences with GTA-research (Wester, 1984; 1995) and with the development and use of the computer program Kwalitan (Peters, 1994) developed for qualitative research according to GTA-principles. The aim of this approach is the development and production of a theoretical framework as described above. It can be applied in cross cultural research, too, if theory development is part of the research objective. But the process of theory development is a complex one, and that is one of the main pitfalls in GTA-research. Especially the lack of an elaborated research problem and/or an articulated analytical framework and/or systematic observation procedures (positively referred to as 'the freedom of the researcher'), may easily get the researcher into trouble. The analyst fails to see the cohesion in research activities, continuously goes on developing new concepts, categories and research questions, questions the relevance of the data most of the time and so gets lost in the process of theory-development.

To cope with these kinds of problems, we believe every qualitative analyst recognises, we stress the central role of reflection in the process of theory

development (see above) and cut this process in phases with specific subgoals to achieve.

In this way we define the following phases in the theory development process:

Exploration phase	the researcher defines the research problem and tries to derive as many relevant concepts as possible from the material.
Specification phase	an attempt is made to define more closely, provide arguments for and construct relevant variables of the distinguished concepts.
Reduction phase	an attempt is made to formulate the core of the theory.
Integration phase	the theory is specified in the sense that the observation units (e.g. respondents) are characterised according to the features distinguished, and the relationships between the characteristics are examined.

Before we turn to the discussion of these research phases, the following should be noted. First, not every researcher will aim at formulating a theory within the framework of the four-phase system described above. Some researchers, especially in the field of cross cultural research, may restrict themselves to a comparative description (cf. specification phase) based on *both* common substantive concepts and group specific concepts, others will focus on the description of the key concept (cf. reduction phase) that explains differences. Depending on the aim of the study, the answers to the questions can be elaborated on basis of the procedural steps outlined below.

Second, the phases and steps are not meant to be a rigid model for doing any qualitative research according to the GTA. It is meant as a set of guidelines for a researcher, the decision on 'what to do next' is always up to the researcher and his creativity. As is also stressed by Strauss & Corbin (1990: 58), it is also possible to mix phases, coding procedures, research actions etc. as one pleases. Our model is meant to be a support for any researcher who wants to look back and forth in his activities to develop theory.

Compared with Strauss' work (Strauss, 1987; Strauss & Corbin, 1990) our four phases approach puts more emphasis on: (a) the elaboration of the process of grounded theory-development in phases with the so called *reduction phase* as the most central part in theory development; (b) the elaboration of Glaser and Strauss' coding procedures in concrete analysis steps.

During the analysis activities, described above and elaborated below, the researcher can make use of a number of techniques and tools, that enable the

manipulation of the analysis material, keep track of the analysis, and present the results of the analyses in a compact and orderly way. Important techniques and procedures in the four phases of the qualitative analysis are:

Memoing	As was elaborated above, memos register cognitive development, i.e. the process of elaboration of the analytical framework. Memos are the products of the reflections on research questions, theoretical sampling and observation, and the outcomes of the analyses.
Transcribing	By transcribing the analyst processes the material in one way or another in order to make it suitable for analysis.
Coding	Through coding theoretical components of the analytical framework are linked to pieces of data (segments).
Summarising	Through summarising the analyst produces reductions of the material according to central concepts in the theoretical framework.
Displaying	A display gives an overview (matrix, diagram, table) of relevant passages from the material from one or a few perspective(s) at the same time.
Computering	This role will be further elaborated in the next section.

GENERAL FUNCTIONS OF THE COMPUTER

In the previous part we described the steps a researcher may follow during the interpretative analysis of qualitative material. In this section we will shed some light on the way the computer can be used to support the analytical actions of the researcher. Before we will go into the four analytical phases in detail, we will first examine the general functions a computer can fulfil in an interpretative qualitative analysis. In the last decade, several computer programs have been developed as a tool for the qualitative researcher, e.g. The Ethnograph (Seidel, 1988), Nudist (Richards & Richards, 1990), Qualpro (Blackman), Hyperqual (Padilla), Atlas/ti (Muhr, 1992), Kwalitan (Peters, 1994). An overview of applications and features of computer programs for qualitative analysis can be found in Tesch (1990) and Fielding & Lee (1991), Miles & Huberman (1993) and Weizman & Miles (1994).

Before talking about the functions of a computer for a qualitative analysis, it is necessary to emphasise that the computer can not perform analyses. This

task, consisting primarily of reading and interpreting, is reserved for the researcher. The computer can, however, fulfil a number of other functions which are classified into seven categories.

(1) *Entering and filing the material*
    This function is almost self evident, since most data entry is performed on (personal) computers. This is an important step, especially in qualitative analysis, where the material used is not presented in a form which is immediately suited for analysis. Interviews are mostly taped on audio cassette, observations are recorded on video or written out roughly, and even written material is usually not directly suitable for the analysis, as the originals may not be used without taking a number of precautionary measures. And in addition, saving the material on disk encourages an efficient archiving thereof, which is especially important in an extensive project. Various printouts can then be made later from the files.
    In interpretative qualitative analysis the entering of the text is not merely a matter of typing the text, but in fact you are making a transcription. We will come to this point in a later section on the preparatory activities.

(2) *Structuring the material*
    Although qualitative data material is generally highly unstructured, this does not mean that it is impossible to introduce a structure of any kind into this material. By applying the principles of so-called relational databases it is possible to save the information available per analysis unit and the information added to this by the researcher in such a way, that an optimal structure of these data is achieved. This amounts, in fact, to splitting up the available information into small units, each with a fixed structure, which gives the possibility of saving these elements in an efficient way and of retrieving them quickly and easily.

(3) *Ordering the material*
    In qualitative analysis the ordering of the material is obtained by assigning codes to segments of texts. The computer can efficiently keep track of which codes were assigned to each of the segments. In addition, the program can quickly make an overview of the codes already used in the analysis. In addition, the computer can be of great help in establishing the relations between codes.

(4) *Selecting parts of the material*
    Storing, structuring and ordering qualitative material is important, but if there is no possibility of retrieving the material all effort is useless. One

of the features of computer applications in qualitative analysis, that saves
the researcher a lot of time and that increases the accuracy of the analysis,
is the possibility of selecting segments that match a number of specified
criteria. These segments can be displayed on the screen or printed, so
they can be used for further analyses.

(5)   *Introducing changes*
One of the characteristics of the interpretative qualitative analysis is that
the conceptual framework develops and becomes definite during the
analysis. This implies, that many of the decisions made by the researcher
during the analysis, are tentative and will have to be changed as the
analysis proceeds and the conceptual framework becomes more firm.
This implies, that in qualitative analysis the researcher, for instance, often
has to change codes assigned in previous steps. The computer can make
these changes faster and more reliable than we can.

(6)   *The administration of the analysis*
The importance of writing memos is already stressed above. Of course,
it is possible to write the memos out on index cards and to file these away
in an index card box, or to use a word processor or a database program
on your computer. Most computer programs, however, offer the
possibility of creating and editing memos, and of saving the memos in so-
called memo files which can be immediately accessed, either for
consultation or for editing. In Kwalitan the four different types of memos,
that were distinguished in the first part of this article, can be handled.

(7)   *Preparing the material for a more structured analysis*
At a certain moment in the analysis, the researcher will feel the need to
conduct systematic comparisons within and between cultural groups. At
that moment there is a need for some type of table or matrix, in which
qualitative information from cases ordered per cultural group can be
compared. The computer can help the researcher in creating these
matrices from the information that was included in and added to the
qualitative material in the previous steps of the analysis.

PHASES, TECHNIQUES AND COMPUTER USE

After the short description of the general functions a computer can fulfil in
qualitative analysis, we will now turn to the process of qualitative analysis. We
will follow the four phases that were discerned in a previous section (preceded
by a preparatory phase), and for each phase we will describe some techniques

the researcher can use. The support computer programs for qualitative analysis may give will mostly be illustrated on the basis of the program Kwalitan.

The Preparation

A qualitative study begins with the formulation of a temporary problem statement based on theoretical assumptions of the researcher, and on what is known about the field of study. These supply the sensitising concepts that direct observation and analysis activities in the exploratory phase. This exploratory phase is dedicated to finding as many concepts and categories as possible that contribute to offering an overview about what is relevant in the field in view of the problem statement.

It is true that the researcher uses a rough theoretical framework to formulate research questions, and has elaborated these concepts in terms of topics for the interview, but this framework is still open and has many gaps. These gaps and uncertainties mainly concern aspects related to the reconstruction of the actor perspectives and the relation of the answers given from that perspective to the sensitising concepts used to formulate the problem statement.

Preparing qualitative material includes, as indicated above, the entering of the material (the text) into a computer file. But there is more than just typing or scanning the text. The activities we refer to are called the transcription of the raw material. The researcher has to determine in what way and how comprehensive the qualitative material is entered into a file. In the case of interviews this seems rather simple, but in fact you have to decide whether to include all hesitations, silences and disturbances in the transcription, or to make a smooth story out of it. If your material consists of video taped observations, the decisions you have to make become more complicated, because visual information has to be transformed into textual information. But even if your raw material consists of existing documents (e.g. the minutes of a series of meetings or a policy report) you have to decide whether to include all texts in the transcription, or to confine yourself to those parts you think are important. Besides that, in the case of interviews or existing documents you theoretically have the choice between entering the text literally or to enter your summary of it. Whatever decision you make, it will be very hard, and sometimes even impossible, to undo these decisions concerning the transcription afterwards.

Another decision concerns the question whether you should or should not pre-structure the raw material during the phase of transcription. Some qualitative researchers hold the position that at the start of the analysis you can or may not add any information to the material in order to introduce structure. Structuring the material is an analytical act, that should not be done during a

pre-analytical phase. Although we endorse this point of view, we nevertheless decided, during the development of Kwalitan, that preceding the actual analysis the researcher has to divide the text into segments, that will be the unit of analysis in the exploratory phase. In order to avoid the researcher from being ahead of the actual analysis, it is advised to divide the text into segments at logical positions in the text, for instance, when the interviewer asks a new question or, in case of an existing document, at the start of a new paragraph or section. This division of the text enables the researcher to introduce some structure in the material, that will facilitate the start of the interpretative analysis. In most programs it is always possible to change the division of segments afterwards, if the results of the analysis (i.e. the researcher's increased knowledge of the subject) urge you to do so. There should be no limitations on the number of segments nor on the length of each segment.

In the case of Kwalitan we decided not to include any option for these preparatory activities in the program itself; word processors have all facilities needed for this task, and it will take users unnecessary time and effort to learn a new built-in word processor for entering the data. Instead, Kwalitan imports the material after it is entered in a file and saved in ASCII format.

After entering the qualitative material into a computer file and after indicating in this file where each segment starts, Kwalitan will modify this file in such a way, that all information is stored in a highly structured way. This new structure is referred to as the *work file*. This work file is made up out of a number of different files, each containing a bit of the information.

Like many other programs Kwalitan has an option to create an inventory of the words occurring in the text, along with their frequencies. Although such quantitative data hardly contribute to the interpretative qualitative analysis, this list can be very useful to get acquainted with the kind of words/jargon the respondent uses. You can, for instance, use it to look up in what way the interviewee refers to a certain object.

The Exploration Phase

The main analytical activity of the researcher in this phase is indicated as *open coding*. This activity consists of reading the text line by line and coding per segment the occurrence of relevant concepts. Now the analyst formulates as many codes as may be relevant in view of the research questions. Open coding is thus focused on the formulation of many substantive concepts, so that the researcher may elaborate a framework that fits the interview data.

One may apply several techniques to discover or formulate substantive concepts (codes). The most important one is *focused reading*. Here the analyst

reviews the concepts and categories that were important during the formulation of the problem statement, the selection of respondents, and the construction of the topic list. With this preliminary framework as a background, the interviews are read in the light of the question: what is said about the topic (concept, category) concerned. Concepts and categories are added to the interview segments where relevant, and more specific codes are formulated and added indicating what is said about the concepts and categories. This way of reading is strongly related to the perspective of the researcher.

A second technique is *reading by summarising*. Here the analyst reads each segment on its own and tries to reformulate the tenor of the text. He selects the main terms in the segment as codes to be added to the segment. This way of reading is more related to the perspective of the respondent or document.

A third technique is called *scanning*. Here we explicitly make use of the possibilities of the computer as a word processor to get an overview of the word-use of the respondent or in a document. Most words used are of course not specifically relevant for the research topic, but some are. These words may be selected from an overview or list of words and automatically added as codes to the segments in which they occur. In this way these specific segments may be sorted for a further focussed reading.

In cross cultural research the exploratory analysis is done for every cultural group. From each group a few cases are selected and combined in a work file. By applying the reading techniques mentioned above on the documents in each work file group specific and general codes are developed.

In this way, the exploratory phase yields a huge amount of codes, some of which are related to the temporary framework the researcher applies in the analysis. This phase of field exploration may be concluded if renewed reading (or new data) does not bring up new lines of analysis (c.f. saturation).

Figure 2 displays an example of a segment and the codes assigned to this segment. The example is from our analysis of Desmond Tutu's preaching (in Pieterse, 1995).

After assigning the codes and entering them into the work file, you will feel the need to examine the codes you have made up so far. The computer may provide you with an alphabetically ordered list of the codes assigned to any of the segments in the work file. This list will enable you to check for typing errors and to see if you have created and assigned synonyms for the same phenomenon. After checking this list, you can correct errors or introduce other changes.

The importance of describing your activities and decisions in memos was stressed above. In the exploration phase you will mainly create concept memos, in which you define the meaning of the (most significant) codes you have
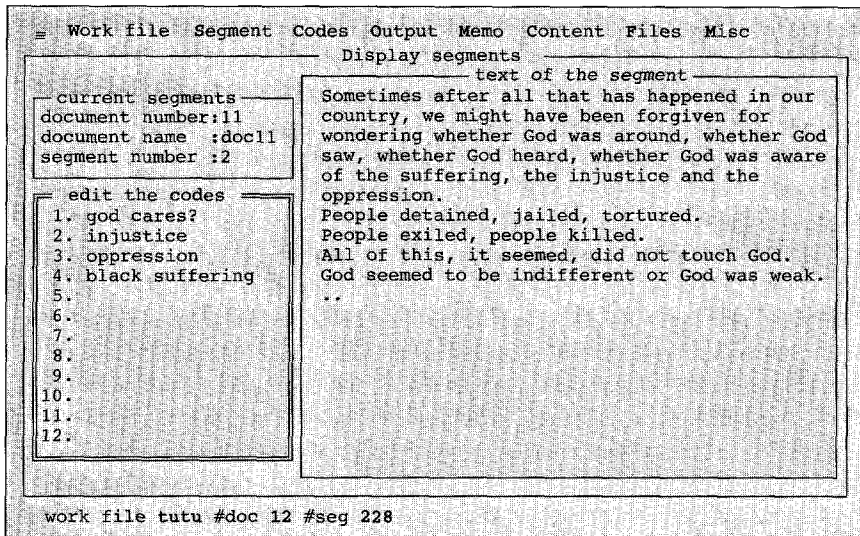
```
 ≡  Work file  Segment  Codes  Output  Memo  Content  Files  Misc
 ──────────────────────── Display segments ────────────────────────
                         ──────── text of the segment ────────
  ┌─ current segments ──┐ │ Sometimes after all that has happened in our
  │ document number:11  │ │ country, we might have been forgiven for
  │ document name :doc11│ │ wondering whether God was around, whether God
  │ segment number :2   │ │ saw, whether God heard, whether God was aware
  └─────────────────────┘ │ of the suffering, the injustice and the
  ┌─ edit the codes ════╗ │ oppression.
  │ 1. god cares?       │ │ People detained, jailed, tortured.
  │ 2. injustice        │ │ People exiled, people killed.
  │ 3. oppression       │ │ All of this, it seemed, did not touch God.
  │ 4. black suffering  │ │ God seemed to be indifferent or God was weak.
  │ 5.                  │ │ ..
  │ 6.                  │
  │ 7.                  │
  │ 8.                  │
  │ 9.                  │
  │10.                  │
  │11.                  │
  │12.                  │

 work file tutu #doc 12 #seg 228
```

Fig. 2. Display of a Segment from a Document.

assigned. In addition you will have to write theoretical memos, in which you elaborate the theoretical frame work and formulate hypotheses.

To this rough description of the analytical procedures in the exploratory phase we want to add some notes on the problems the researcher may face.

(1)   *'Loss of time' because of the transcription work*
      Transcribing interviews requires more time than many analysts have planned and it results in a huge amount of paperwork that almost inevitably frustrates analysis. This raises the question as to whether there is some way to economise on this work. That is possible later, in the integration phase, but not in the initial phases of analysis. A first rough transcription from tape to floppy may be executed by an assistant, but the analyst himself should review the tape to make corrections to the basic transcript. Transcribing is not 'lost time', it is a first step to becoming acquainted with what is in the interviews. Moreover, transcribing is necessary to add to the text some explicating remarks if it is incomprehensible or confusing. After transcribing and correcting the analyst feels again at home in the interview and may start to create a profile card that gives a rough sketch of what the respondent thinks about central topics.

(2) *The time-consuming exploratory reading*
     Analysis according to the above-mentioned techniques takes a lot of time.
     If you planned some fifty interviews and all of them should be read in the
     ways described above, you can easily imagine that your time-budget is
     not large enough. Besides, it would yield such a large quantity of coded
     transcripts that the specification phase, too, would require a lot of time.
     The solution for this problem is that the exploratory analysis is executed
     on a small, strategically chosen set of interviews. This means that after
     interviewing some 15 respondents in the exploratory phase, about 5
     interviews are selected for exploratory reading, on the basis of profile
     cards written immediately after the interview, so that a wide variation is
     included regarding data pertinent to the central topics. After all, the
     objective in the exploratory phase is the development of as many
     substantive codes as possible, and you will not need all documents for
     that.

(3) *The danger of fragmentation*
     Line by line analysis, the reading and coding of segments, and the option
     to compare segments across the interviews may lead to the danger that
     the texts are read out of their context: the interview with a specific
     respondent. The solution here is, that one regularly consults the
     respondent's profile card and that this profile card is amended in each
     phase of analysis as to the view of the respondent on central topics.

(4) *Directing the analysis*
     The main problem in the exploratory phase is that the analyst reads and
     executes more and more interviews with interesting cases, interviews that
     may lead to still more new codes and that old codes are specified
     continuously, in short that the process is never ending. Glaser and
     Strauss' saturation principle (1967: 111), that is to keep interviewing and
     analysing until no new codes arise, does not work as a practical principle.
     The analyst must direct the process and that is done by taking ample time
     for reflections on the findings, in addition to interviewing and exploratory
     reading. Reflection is stimulated by an overview of codes and the writing
     of memos on the relations between categories within the analytic
     framework. The main clue is Strauss' adage "think theoretical," which
     allows recognition of relationships within the framework.

The exploratory phase can be concluded if the analyst is able to formulate a
preliminary and rough answer to the research questions, and so can formulate
which topics, concepts and categories are central in the study.

The Specification Phase

The specification phase is directed at specifying the concepts and categories formulated in relation to central topics. The analysis should lead to the ordering and elaboration of the codes formulated in the exploratory phase in such a way that the analyst gets a framework to describe the aspects that are important in the field in view of the problem statement.

The analytical activities of the researcher are denoted as *constant comparison*. Based on the results of the exploration phase and of the reflections on these results (memos), the researcher chooses a limited number of codes that seem to be important and have the potential of becoming a key concept. For each key concept, all segments to which the key concept was assigned have to be put together in order to enable the researcher to look for similarities and differences between the segments; based on these comparisons, the researcher is able to elaborate a key concept. The selection of segments and the comparison of these segments have to be repeated several times for each of the central concepts.

The central topics, assigned in terms of main codes to the interview segments, are the points of departure. The computer can select all segments with a specific main code and present all the additional codes that accompany the main codes in the segments. Reflection on the overview of additional codes belonging to the field of a main code should lead to the formulation of the diverse dimensions of the topic the additional codes are referring to. Next, these dimensions are used to formulate variables that describe differences and similarities between respondents as to the central topics of the study.

When these kinds of variables are elaborated for all central topics, the specification phase is finished. The analytic framework has been elaborated sufficiently to give a descriptive overview of the most central topics in the study.

In this analysis phase the computer may be used to specify criteria for selecting the segments, i.e. to create a filter. As you can see in the example of a filter in Fig. 3, you can specify several types of criteria in the filter.

The most important search criterion is a list of one or more codes. The computer will display (or print) only those segments, that match the specified criterion. If you specify two or more codes in a filter, you can combine the codes by any of the logical operators 'or', 'and' and 'not'. Other specifications you can make in a filter concern strings of text (words), that have to occur in the text of the segment, and the segment number or the document number. At the right bottom of the Kwalitan-window you can make a selection on the basis of additional information, that is included in the identifier of a document (this

```
=  Work file   Segment  Codes  Output  Memos  Content  Files  Misc
                         Filter and display scenes

                       composition of the filter
        codes          words in the text
  1. god cares?         -->
  2. +justice
  3.                    document numbers
  4.                    -->
  5.
  6.                    segment numbers
  7.                    -->
  8.
  9.                    specifications in the document identification
 10.                    -->

                       enter up to 10 codes



  work file tutu  #doc 12 #seg 228
```

Fig. 3. Creating a Filter.

identifier may contain, e.g., the age or the sex of the interviewee). In the example of figure 3, only those segments are requested, to which both 'god cares' and 'justice' are assigned; the other types of criteria are not specified.

Selecting segments enables you to concentrate on the similarities and differences between segments, and, doing so, to get more information about the code in question.

Another important task for the researcher in this phase is to see if it is possible to order the codes according to a dimension and to ascribe them to one of the poles of that dimension. In fact the researcher tries to generate a hierarchical scheme of the codes. For this purpose you can use a list of codes that is generated by the computer program. This list looks the same as the list that was mentioned in the previous section, but this time the list only consists of the codes that are assigned to the segments to which the central concept was also assigned. In other words, the list displays the codes that co-occur with a specified code, in this case one of the central concepts. This list enables you to look for dimensions and to distribute the codes over the poles of these dimensions. A second tool for this task gives you the opportunity of creating a tree in which the hierarchical structure of the codes is displayed. This hierarchical structure is included in the work file. This option not only provides you with a visual representation of the hierarchical structure, it also facilitates the creation of filters, because you do not have to fill in all the codes that are
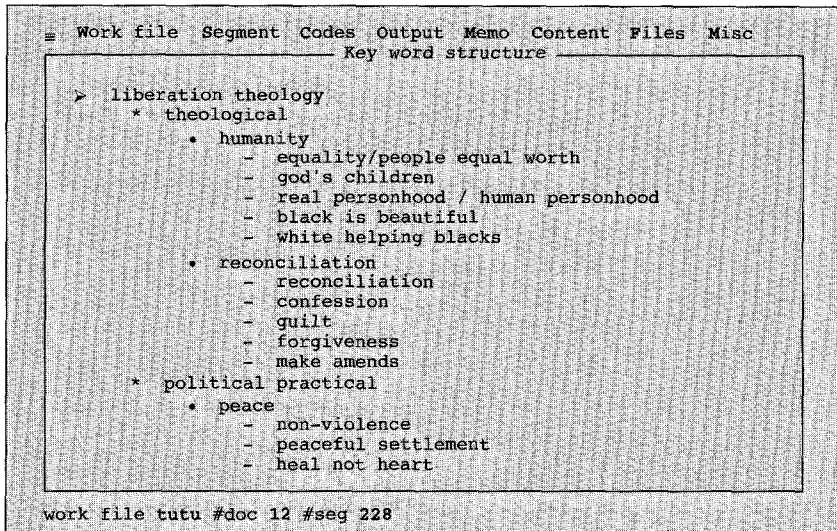
```
≡  Work file  Segment  Codes  Output  Memo  Content  Files  Misc
                        Key word structure
  ➤  liberation theology
        *  theological
              •  humanity
                    -  equality/people equal worth
                    -  god's children
                    -  real personhood / human personhood
                    -  black is beautiful
                    -  white helping blacks
              •  reconciliation
                    -  reconciliation
                    -  confession
                    -  guilt
                    -  forgiveness
                    -  make amends
        *  political practical
              •  peace
                    -  non-violence
                    -  peaceful settlement
                    -  heal not heart

work file tutu #doc 12 #seg 228
```

Fig. 4. The Hierarchical Structure of Codes.

part of one of the branches of the tree, but it will suffice to enter only the 'parent' code. An example of the tree is displayed in Fig. 4.

As always your reflections, ideas, decisions, etc. have to be put down in memos.

The technique of constant comparison deals with comparison of segments within cases resulting in case or group specific codes. Characteristic for cross cultural analysis is the comparative analysis of cases, i.e. within as well as between cultural groups. After common and group specific codes are elaborated into dimensions for comparisons, a systematic comparative analysis of cases from different groups is possible. What the analyst needs now are summaries of the 'score' of each case on the dimension for comparison. In addition to this rough description of the method of analysis in the specification phase we want to make the following notes concerning the problems the analyst may meet.

(1) *Incomplete interviews*
     At the end of the specification phase the researcher is able to define which topics should be discussed in the interviews and which concepts and categories are linked to the topic. This gives the researcher the opportunity to give precise instructions to the interviewers so that a part

of the rest of the interviews may be contracted out. A second implication is that the researcher now understands some of the gaps in the interviews carried out so far, in view of the elaborated analytic framework. That is a consequence of the start of the study: the more open the study starts, the more gaps in the provisional framework, the greater the chance that topics have not been thoroughly pursued in view of the final analysis. Some of the respondents will need to be interviewed again on these topics later in the integration phase.

(2)   *The necessity to work free from codes*
The aim of the specification phase is elaborating the perspective of the researcher. This means that the codes from the exploratory phase are not all equally important. What is more, the codes themselves are not important, they are just tools in elaborating the analyst's perspective. At the end of the specification phase the analytic framework contains terms (concepts, variables, categories) that only partly coincide with codes used earlier. Moreover, some of the codes used in the exploratory phase will not return, because they do not refer to what the central topics are later in the study. So the analyst should release and surpass the codes used in order to prevent the analysis to become merely a bureaucratic completion of loose ends related to all codes.

(3)   *Substantive literature*
In elaborating the analytic framework the analyst needs a wider perspective than the observations as such. At the start of the study he formulated the problem statement on the basis of a rough overview of the field under study extracted from literature surveying the field. In the specification phase the researcher needs specific literature related to the field, that provides the possibility of investigating the relevance of the substantive concepts outside one's own observations, and of solving problems with concept definitions. Moreover, literature may suggest concepts which help to formulate relevant distinctions found in the data.

(4)   *Taking time for reflection*
In the specification phase the analyst experiences all the advantages of analysing with the help of the computer. In the comparative analysis the analyst uses selections from the data for comparisons in view of the formulated categories (cf. Strauss' axial coding). While reading the interview segments not summarising but interpretation of the text is the central issue. This means that all kinds of analysis work is alternated with cognitive work, in which the insights and creativity of the analyst play a

substantial part. Reflections on the analytic framework and the doc-
umentation of the developing insights in memos constitute the main
cognitive activities during analysis in the specification phase.

The Reduction Phase

In the reduction phase the core of the developing theory is defined to structure
the elaborated variables and their interrelations. In the specification phase the
analytic framework has been developed 'vertically', in the sense that for every
research topic the structure of the relevant concept and related dimensions,
variables and categories has been elaborated. In the reduction phase the theory
is developed 'horizontally', that is the relationships between topics, and the
relationships between concepts and between variables.

The core of the theory may be one of the central concepts, e.g. a typology
the analyst has formulated that has many interrelations with other variables. Or
the core may be a process that describes changes and developments in a central
topic, giving order to the rest of the developed variables.

Although the reduction phase is defined here as a separate phase, it is often
the case that the core of the theory has already emerged in the previous phases.
In that case analysis in the reduction phase is dedicated to testing whether the
core concept indeed brings order into the data.

In the reduction phase analysis mainly consists of reflections on the basis of
specific selections from the data, overviews and tables. There are several
strategies which can be used to track the core of the theory. Besides the
scanning of memos and of literature, one may study the profile cards of each
respondent individually to examine how, in each case, topics are related.
Another strategy is to select from all interviews those segments that show a
relationship between topics. Or one may produce overviews of the 'scores' of
the respondents on several topics to search for interrelationships. In cross
cultural research, the researcher looks for patterns within cultural groups, that
may refer to differences between groups, or processes common to all groups.

The reduction phase uses the tools that were described above. Making
printouts, entering codes, creating filters and selecting segments make up the
time consuming activities of the researcher, that can be taken over by a
computer program.

The purpose of these analytical activities is to search for the core of the new
theory by establishing the relations between the main variables. A handy tool
in this phase is a graphical environment, in which you can draw and manipulate
a schematic representation of the relations between the variables. Some of the
computer programs mentioned above (e.g. Atlas/ti) have an option to create and

manipulate graphical representations of the relations between concepts. You can also use a separate computer program for drawing schemes, but this has the disadvantage that the information is not linked to your work file. And for those who are not familiar with graphical computer programs: a piece of paper and a pencil will do too.

Looking for important variables and their relations implies the creation of all sorts of matrices and tables. When referring to matrices, one should not think of the well known matrices the quantitative researcher is so fond of, but of qualitative matrices, in which the cells are made up out of qualitative descriptions. Miles & Huberman (1993) give a large number of examples of this kind of matrices and of the way you can treat them. In compiling qualitative matrices the computer can be a great help, since you can select rather precisely those text segments that give you the desired information.

Another important analytical activity of the researcher in this phase consists of a systematic and thorough study of the memos you have been writing during the process of the analysis. You have to recapitulate and summarise the ideas you have written down in the memo's. If you would like to analyse your own memos you may transform your memos into a work file, that you can analyse in the usual way.

The reduction phase is concluded if the core concept or process is formulated and its relationships with other topics is made clear. In addition to this rough overview of the analysis in the reduction phase, the following notes should be made.

(1)   *Change in the level of analysis*
      In the previous phases the analyst has been occupied by coding and comparing segments, on the level of the data (except for theory memos and profile cards). In the reduction phase the situation is different; now the analysis focus on the case level (respondent, document). The relevance of the elaborated framework can be tested by summarising the interviews in terms of the framework and to check out whether all relevant information has been recorded.

(2)   *The relevance of theoretical literature*
      In the reduction phase the analyst needs an overview of relevant theoretical perspectives on the subject under study to possibly borrow the core concept from accepted theories. Besides this, the analyst is focused on the definition of the core concept and the relationships of the core with other concepts, and here too, theoretical literature may provide workable suggestions.

The Integration Phase

The integration phase is dedicated to defining the theory precisely, testing the relationships between the core concept and other concepts in the theory through new data, and formulating an answer to the problem statement, illustrating it with observations from the data. The analysis has the characteristics of a systematic research on the basis of a stable and complete analytic framework.

The final analysis takes place on the profile card of each case, which contains summaries of the central topics in terms of the framework. Now, it can be tested for every case whether the core concept is capable of reflecting the core of the respondents perspective, and the relationships of core and central concepts for the research group as a whole. For cross cultural research this means a systematic analysis of the variations within and between cultural groups.

The last step is the description of the theory in a research report, the basis of which can be found in many methodical and theoretical memos the researcher has written during the whole process of analysis.

This phase entails the specifying of the theory, in the sense that the observation units (respondents) are described and characterised according to the variables and relationships that make up the (new) theory. Matrices, tables, summaries and all sort of overviews are the tools the researcher will need in these phase. In addition to the possibilities described above, the computer may also help to create quasi quantitative matrices, in which for instance is indicated if a code was encountered in the material of a respondent, or in which is counted in how many segments a specific code was encountered. These matrices are written in such a format, that you can execute rather sophisticated analyses, like homogeneity analysis (Homals) or analysis of correspondence. These analyses are meant to get a better understanding of the relations between the variables and to be able to divide the units (respondents) into categories.

Concerning the process of analysis in the integration phase the following notes are of importance.

(1)   *Interviewing 'former' respondents again*
        Because the analyst's framework is completed and made definitive at this
        moment in the research process, it now becomes clear what the
        restrictions and gaps are in the interviews from the initial phases. As a
        consequence, these respondents should be interviewed again which may
        provide important insights. On the one hand it may turn out that the
        respondents do not speak about the interview topics in exactly the same
        words, which may be a good test for the analytic framework to go beyond
        this 'everyday variation'. On the other hand it may turn out that some
        respondents' answers have changed in regard to the topics studied. Then

the question is whether the theory can explain why such a change has occurred.

(2) *Analysing is writing*
Most analysis steps are recorded in some way or another in some written product. While writing the research report, the researcher may derive illustrations from these products, like the many theoretical and methodical memos, the coded interview segments and the profile cards of respondents. The writing of the final report demands precise formulations, legitimisation of research design decisions and grounded interpretations of the interview data that should be consistent with each other. In this way the writing of the research report in fact is the final step in the process of analysis.

CONCLUSION

Qualitative analysis is a laborious process. We have presented a stepwise procedure to organise this complex process, that may also be applied in cross cultural case studies. The computer can be of great help in manipulating the data. One can hardly imagine a situation in which a researcher does this kind of analyses without the support of a computer. We foresee a further elaboration of the methodology of qualitative analyse; the developments in the area of computer software will enable refinement of the methodology for qualitative analysis and will stimulate the development of new techniques for qualitative analysis.

NOTE

1. We are aware of the fact that many researchers are female, but for the sake of the readability of the text we speak of he and him when we refer to the researcher.

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CROSS-CULTURAL CASE STUDY WORK. IN SEARCH OF INTERNATIONAL UNDERSTANDING IN EDUCATIONAL RESEARCH

Michael Schratz

Editors' Commentary: *Michael Schratz begins his chapter with the premise that whilst quantitative research is likely to remain broadly similar cross-culturally, qualitative research is likely to differ considerably. From the outset, Schratz introduces the notion of the cultural specificity of cross-cultural research. Consequently, effective communication between qualitative researchers engaged in cross-cultural research demands an understanding of the cultural biases, which shape scholarly endeavour within different countries. Schratz stresses the importance for researchers to travel, to communicate, to attend conferences and to become familiar with what he terms the 'Socio-cultural Hardware' of different societies. Drawing on examples of collective memory work, Schratz discusses discourse patterns, conscious and unconscious issues, which shape intercultural case study.*

TOWARDS AN INTERCULTURAL PERSPECTIVE

Through the enhancement of international cooperation and mobility in academic education and research training many students spend at least some time of their university career in different countries taking part in cross-cultural

curricular activities. There they not only try to enrich their study experience in their particular subject areas with new curricular items, but they are also confronted with different sorts of research experiences. Whereas the 'official' academic curriculum is usually made explicit in international study programs and student exchanges, the 'hidden curriculum' often remains concealed – even for a longer stretch of time once students have arrived at their host institution. Gerholm (1985) differenciates between two main categories of tacit knowledge, which is established at a research institution.

> One of them is the knowledge that has grown out of long experience in the discipline. It is a practical, almost subconscious, knowledge or competence that the department elite fully masters. The most important ingredient is the knowledge and command of the repertoire of scientific discourses. The other category of tacit knowledge is generated by the students themselves as they try to make sense of what they are experiencing in the graduate studies program. Like the former type, it is likely to be used as a guide tor action. And for an understanding of what goes on in Academia they are both ot great importance. (Quoted after Becher, 1989, p. 26).

Tony Becher (1989) fittingly compares university campuses with different tribal systems. They have their territories and boundaries, their fields of competition and their pecking orders within and between them, their tacit knowledge and hidden agendas, and their specific patterns of communication, publications, division of labour, hierarchies and careers (cf. Huber, 1990, p. 24). As part of their socialisation into an academic discipline, everybody entering university life has to find their way into the scientific community by complying with its fundamental rules:

> To function smoothly within the group of teachers, fellow students and secretaries, the student needs a considerable amount of know-how. Most of it will be acquired slowly through the interaction with others and without anyone ever making a deliberate effort to teach the newcomer the rules of the game. Nonetheless, failure to comply with these implicit rules will undoubtedly affect the student standing within the group (Gerholm, 1985, quoted after Becher, 1989, p. 26).

A student usually learns his or her way into the official rules of a university department through introductory courses and the tacit knowledge through discourses among students and staff. If students go somewhere else to fulfill international course credentials for their study requirements they often find it difficult to adjust to the new and different 'tribal systems'. However, this very much depends on the prevalent research paradigm. It is not difficult to play chess according to the rules if the game is played in the same way throughout the different scientific communities. Similarly, research which is based on quantitative measurement, variables, experimentation and operationalization can be performed according to the same rules all around the world. If a

(student) researcher joins a different scientific community he or she will not have great difficulties in finding his or her way into the research agenda.

The more qualitatively oriented approaches are, the more diffcult (student) scholars find it to understand the workings of research in a particular scientific community if they go to different universities. They often feel as "professional strangers" (Agar, 1989) who are familiar with the academic discipline but do not know the 'hidden agenda' of how to behave there. It is as if a chess player is familiar with the conventional chess board but the rules do not apply any more in the same way as he or she learnt to play it in his or her cultural setting. Therefore, in international communication among students and scholars, it is important to understand the cultural bias of academic life.

This cannot only be done by reading research literature from the scientific community one intends to go to, because that only conveys part of what I call the socio-cultural 'hardware' of an academic institution and mainly represents the chess board I alluded to above. Moreover, it is important to understand the discourse patterns prevalent. They form the 'software', so to speak, of how research is put into practice, in particular in scientific communities where research is built on qualitative voices rather than statistical data and the criteria of objectivity, reliability and validity (cf Schratz, 1993). Therefore it is important for international research work to be more sensitive to the understanding of what is not immediately known to one another.

Usually it is only the tip of the iceberg – in our case aims, judgements, laws, tasks, research practice, research instruments, research culture, justification – which is known, whereas the other parts of intercultural understanding in research work are hidden under the surface of the unconscious mind (cf Larcher, 1999: 184). Figure 1 gives an idea of what is conscious and what is unconscious.

Whereas the visible parts of the iceberg are usually communicated in the research discourse (through conferences, literature etc.) and therefore available across national boundaries, the hidden parts belong to the tacit knowledge of the insiders. They form the kind of 'songlines' of the tribal research culture. The unconscious part can vary individually and socially according to ones own experiences. Individual and/or collective self-reflection can enlarge the area above the imagined dividing line between the conscious and the unconscious. However, individual and collective repression can reduce the conscious part of one's culture.

The cultural gap between scientific communities is not static, but leaves some space for negotiations between the conscious and unconscious state. Therefore, it is important for intercultural research work to enlarge the conscious part in the research process. To do so, I find it useful to look at

Fig. 1. The iceberg concept.

intercultural case study work as a useful way of developing one's own perception of the relationship between the conscious in the respective research cultures.

INTERCULTURAL CASE STUDY WORK

Lawrence Stenhouse, who was well known for his curriculum development work, later in his research career became known for his writing on case study and its use in educational research. In his argument for (and against) case study work he writes:

> I take it that social science or social scholarship is a form of institutionalized cooperative endeavour to extend and deepen understanding. It deals in public knowledge, public critique, public techniques, and through them creates a tradition, accessible to people of reasonable ability and industry, within which contributions to knowledge and under-standing can be made. By means of a collaborative public structure it enables fairly humdrum people to make contributions to human understanding of a kind which pre-scholarly societies received only from the outstanding visionary or sage. Social science is democratic in the sense that the community of social scientists shares knowledge in such a way that the work of the less able is strengthened by the work of the more able, and that critical principles are accessible in the light of which the most authoritative scholars can be called to account (Stenhouse, 1993: 23–24).

Here Stenhouse makes a strong case for cooperative endeavors within scientific communities, which often produce community rather than deepen understanding among its members. If we see research work as an international 'cooperative endeavor to extend and deepen understanding', it is even more difficult to create a tradition accessible to members of different research tribes. Case studies are a useful method in making the different cultures accessible, since:

> There is a complementary relationship between law-seeking sample study and descriptive case study and that relationship is founded in reality. Neither study feeds merely on the inadequacy of the other. Human life is partly lawful, partly unlawful. In so far as it is unlawful it can be studied in terms of linguistic meanings rather than in terms of operational specifications (Stenhouse, 1993: 23).

Different scientific communities have culture-bound discourse norms. Therefore differences in discourse patterns sometimes operate as a barrier to the exchange of scholarship between two related cultures.

> If English- and German-educated scholars do apply different formal criteria to judge the acceptability of academic writings, and cultural differences make them susceptible to such judgements, international academic exchange and cooperation may suffer (Clyne, 1987: 215).

It seems to me, the study of cases with an intercultural perspective helps in deconstructing some of the unconscious parts in the research iceberg referred to above. The very fact that case studies have had different traditions in different countries calls for particular attention. Whereas in English-speaking countries case studies have made a considerable contribution to the corpus of knowledge and practical wisdom about education, in other countries – like the German-speaking scientific communities – case studies have been regarded as unscientific and therefore not had such an influence on the research agenda at large (cf Kelchtermans, Schratz & Vandenberghe, 1994). Clyne argues that knowledge is idealized in the German tradition, and consequently texts by Germans are less designed to be easy to read. For him, their emphasis is on providing readers with knowledge, theory, and stimulus to thought. "In view of the importance of international communication between scholars, it is vital for scholars to understand the cultural basis of many discourse patterns." (Clyne, 1987: 239)

Intercultural case study work can create an intercultural experience which helps in understanding the cultural bias of one's own research approach and that of the other culture involved. I introduce the Johari window (cf. Luft, 1963) as a heuristic instrument which can help in dealing with the complexities of intercultural case study work.

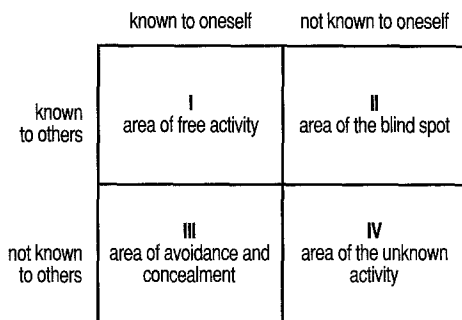The four quadrants can be described in the following way:

	known to oneself	not known to oneself
known to others	I area of free activity	II area of the blind spot
not known to others	III area of avoidance and concealment	IV area of the unknown activity

Fig. 2. The Johari-Window.

Quadrant I, the area of free activity, which gives an insight into the behaviour and motivations which are known to oneself and to others.

Quadrant II, the area of the blind spot, characterizes the area where others can see things which oneself does not know about.

Quadrant III, the area of avoidance and concealment, represents aspects which one knows about but which are not passed on to others (e.g. things which are delicate).

Quadrant IV the area of the unknown activity. Neither oneself nor others know about certain hidden motives. These are often repressed emotional aspects which influence the relationship among people. Nadig & Erdheim (1984) tried to show how scientific experience is usually destroyed by the academic milieu. They argue that when we stick to academic routines emotional parts are suppressed into the subconscious, from where they destroy the liveliness of academic study and research in an uncontrolled and destructive way. In order to pay more attention to the more disturbing qualities of everyday life, we have to see research as an interactive process in its socio-cultural context.

In order to pay more attention to the dynamics of those processes in research in international activities it will be necessary

- to create a better understanding of what it means to do research in intercultural settings

and

- to find appropriate methods to serve this overall understanding.

To do so we can use the Johari Window explained above. There it will be necessary to enlarge the area of free activity in quadrant I so that more of the
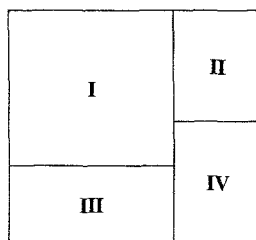
Fig. 3. Aim of Intercultural Case Study Work.

relationships between people or groups become free and open. At the same time quadrants II, III, IV get smaller Most of the reduction in size takes place in quadrant III, then in quadrant II, and least in quadrant IV, as can be seeen in Fig. 3.

The area of the free activity can be enlarged by learning more about the socio-cultural background of the research discussed. Especially in qualitative research the relationship between the individual(s) or group(s) and the researchers themselves matters in matching one's research approach relevant to others. To engage in critical inquiry into the intercultural aspects of case study work it is therefore necessary to analyze the research process from a metacommunicative point of view. Metacommunication is communication *about* how people interact among each other, how successfully communication takes place, how people behave in certain situations and so on. In the use of intercultural case studies this metacommunication can be achieved by means of phases of self-reflection, which offer a good way

- to throw light on the (usually hidden) motives of the individual members of a research group,
- to deconstruct the individual perception of the research process and to decipher the social construction of certain key situations involved,
- to analyze the research process according to certain discursive patterns used in collaborative work,
- to put research findings into a broader context of the wider educational community or even society at large,
- to assess work on the micro-level of educational change for its transforma- tional power on the macro-level of societal development.

In the following I want to give two concrete examples of how I have used case studies to make students aware of the intercultural aspects in doing research. The first example shows the relationship between the context of research and

the researchers themselves from a socio-cultural perspective, which forms the grounding for intercultural understanding in doing international case study work. The second example gives an insight into the use of memory work drawing on educationally relevant incidents in individuals' lives. The examples are taken from the work in a research methodology course which took place at the International Peace University Centre at Schlaining, Austria, and which I taught with Rob Walker (Deakin University, Australia).

PERSPECTIVE ONE: RESEARCH AND ME

One way of relating social theories more to the real world of intercultural interactions, the constructing and organising process of knowledge and theorising, is to see the researchers included in, rather than outside, the body of their own research. If we think of research as constituted by processes of individual and social reflexivity and reciprocity, intercultural understanding of different research agendas depends on how these are communicated among the researchers involved.

In order to make them more aware of the embeddedness of research we have to ask (student) researchers about how they see their research in relation to themselves. Here are three three texts from a methodology class in peace studies which I want to use as concrete examples of this relationship). We first asked each of the students coming from different cultural backgrounds to visualise what research means to them personally, so as to give each of them the chance to associate more freely. Each of them got a sheet of paper to do this Thus the sheet of paper represented the boundaries of life and they tried to fill in the internal anatomy which shows that research is part of their lives.

The first drawing was done by Jürgen from Switzerland.

Jürgen gave the following commentary on his drawing:

It looks like a . . . and to comment on the pictures, so I start at the sheet, it is my consciousness and there's somewhere a centre, what I know best, what is closest to myself, which is represented by the dark dot, and then this consciousness is expanding, covers different areas and has lost spots somewhere, not connected and the other is connected. There are also holes in that extended consciousness of things that are nowhere in the midst of my consciousness, of my knowing, or even the closest to the centre there are things that I do not know myself. So taking now this mind map, how I could call it, mind map of counsciousness. Within this the research would be what I represented with those arrows, the expansion of consciousness of knowing in all directions connecting the different lost spots interconnecting knowing and also filling up the gaps which are the left-overs in the hurrying expansion. Well, that's about it.

Fig. 4. Jürgen's drawing.

The following drawing symbolises András' relationship of research and himself, whose research dealt with the situation of the Romas in Hungary.[1]

Fig. 5. András' drawing.

András commented his drawing as follows:

This is a bridge, a symbol of the bridge, it's a very dynamic, moving bridge and on one side, on the left side of the drawing that's the Roma national flag, and the Romas, at least according to my personal experience, would like to get integrated in society, in Hungarian society in all of the senses one can imagine politically, mainly economically, but in other senses as well. And on the other side of the bridge, this big question mark is me. The only thing I'm really certain about is that I'm uncertain in a lot of senses. So this is a very personal view of the Hungarian society and since we are coexist, I mean I'm a non-Roma, so I'm not a member of the Roma community, but I feel the need to establish links and try to improve our coexistance within this society as much as possible. so I'm trying to do a lot of work, I'm involved in a lot of projects about them. But at the same time, so this is what the arrows towards the Roma community symbolize, but at the same time I always fear, and I'm aware of the impact that by using and by talking about them or by working with them I'm legitimizing myself, my own voice. And these are the arrows which are pointing back towards me, and sometimes these dilemmas, these doubts, these feelings are very strong, so I'm considering just stepping down from the bridge and these are the arrows which are pointing down from the bridge. So this is a very personal and political statement about research how I feel research about the Romas. So there are two little shoes right next to the question mark and these are shoes without my feet in them so I really feel that the shoes should move the steps towards the Romas and, not towards the Romas, but with the Romas in a certain sense should be taken, but my feet are still not there because of the doubts I was just talking about, I just sometimes feel that this is just the way, so whether you can really take steps on this bridge, but still it's there and so these shoes be moving, but I'm . . . sometimes I don't know if my feet will be on means to legitimize my own voice. And now, just finally two words about the bridge: So it's a very personal view of Hungarian society, whereit's just one segment of the society, in fact this is a very, it's hard to say, it's a very unstable bridge, it's filled up with stereotypes, it's moving all the time. You don't know if you begin to take the steps, maybe it's gonna collapse at one point. There were already anti-Roma events at Rosatits [place name] taking place in Hungary, so I'm not sure whether this bridge is those shoes but I really hope so?

The third drawing was done by Onoriode from Nigeria.
This is how she commented her drawing:

This diagram symbolically shows how research has been part of my life. If you look at the diagram you have three, there are four . . . it just tries to tell that, you know, when on our own . . . we don't really know everything, or it's not possible for one person to know everything, there are always people who know more than you or who know the things you don't know. So naturally you need other people, like if you look at it, I have some, I have diagrams of some houses here. If one stays and just isolates oneself in one of these houses, and you don't know much and you don't bother to get [how to fix] your little shell to interact with other people in order to know what they know, because, I mean, it's certain that you need what other people know, you need other people's knowledge to your own to really make ahead with indirectly saying that if you, if you just stay on your own, there is the likelihood you don't know everything, so there's the possibility of . . . and if you decide to stay within . . . if you decide to remain ignorant there's the possibility of . . . and if you

Fig. 6. Onoriode's drawing.

decide to stay within . . . if you decide to remain ignorant theres the possibility of falling apart, but if you get out and get to interact with other people and add to your knowledge you naturally learn more to enable you survive and this is like saying we live in an interdependent world and nobody can do it alone or go it alone. In essence, what I'm trying to say is that one can't really survive on once, you know, putting each native friend with it no man is an island, we always need other people and we need to add other people's knowledge to our knowledge to be able to get . . . to be able to obtain something concrete or make sense at all of what is happening around us.

The drawings which these texts refer to were used as the first cases when working out the contextual background in cross-cultural research work. They served as a prerequisite in setting the scene for further (case study) work. At the same time they also helped in creating an understanding of the different methodological approaches in the countries the participants came from.

PERSPECTIVE TWO: THE SELF AS A SOURCE OF EVIDENCE

Memory-work was first developed by a group of feminist researchers and scholars, mostly from Berlin, who had had some contact or involvement with a tradition of social thought and social action that began, for most of them, with the student movement in the 1960s, who had been members of various women's socialist groups and who generally worked as academics and professionals in teaching and in social science. They were women researchers who found themselves (and felt themselves) to be increasingly alienated from research, alienated in the classic sense of feeling that the process of creating research 'products' left them disconnected from research, both as researchers and, even more, as research subjects. They were dissatisfied with the divorce of theory from everyday experience as well as concerned that the individualistic approach of much of the research they encountered appeared to have the effect of undermining the significance of the collaboration and capacity for collective action that they had gained from their experiences in women's groups.

Memory-work accepts the view that anything a person remembers constitutes a relevant trace in his or her construction of self. Memories, it is assumed, are not direct quotations from experience, but are continually reprocessed through the formation of identity. Certain events from the past become subjectively significant in the process of social construction by which the self is created; in some ways memories play a role in memory-work that is comparable to role that dreams play in psycho-analysis. What is significant about memories is not their surface validity as true records, but their role in the construction of identity. Memory-work has been developed as a powerful research method to reveal the processes of the construction of self by uncovering subjective significance in personal accounts. Since meanings are constituted in actions which are personally relevant and actions constituted in subjectively significant meanings, so memories are coloured by the inter-pretations we bring to the task of envisaging particular incidents in our lives.

First phase: In small groups of four to five the individual researchers each write down a text from their memories referring to the chosen theme. This could be an episode, event or action from one's own experience. However, the text is always written in the third person so as to distance the actor from the person who remembers the experience. The written text should illuminate the scene(s) in as much detail as possible, including even inconsequential or trivial detail, but without offering any interpretation or explanation for what is described. We have found too that the process will work better if we advise

participants to write one of their earliest memories rather than something recent. Normally people will write one to several pages, depending on how long they are given to do so. Ideally they should be given several days. Sometimes, for workshop purposes, we have had people write in thirty minutes or so, but this should be avoided if at all possible. One of the most interesting parts of the process for participants is that the moments they choose to write about rarely pop straight into their heads but sometimes come to them only after several days of thinking around the theme. This process, in which memories are searched, examined and refiled before one is selected for close scrutiny is not wasted time but helps feed later deliberation and discussion

In the *second phase,* the texts are exchanged and analysed among the members of the (original) group(s). It is important to emphasise that this process is very different from that used by conventional discussion groups organised around the reading of a text. Since interpretation does not figure in the text, and since the author has anyway adopted the convention of writing in the third person, there is little heed given to stylistics or to literary or expressive form. The criteria for the analysis of the collected texts are the relevant traces of the social formation process, which are often to be found in gaps and in silences or in the cliches and contradictions contained within the collected written texts. Thus the researchers ask each other (not necessarily the author) for clarification of ambiguities, for more background information on what is missing and the social role(s) of the actor(s) involved in the texts. After having analysed each individual memory text the group looks for similarities and differences between the memories and looks for continuous elements among memories whose relation to each other is not immediately apparent. Each member should question particularly those aspects of the events which do not appear amenable to comparison.

In the *next phase,* the members of the group rewrite their original memory texts, paying particular attention to the questions raised by their co-researchers in the analytical phase. By modifying the texts, the authors are engaged in reflective processes which bring to light new 'data' from their memory which might have been suppressed in their identity formation process and which often suggest reinterpretations of the construction of self. The new memory texts are discussed again among the co-researchers. This time the original versions of the memories are compared and contrasted with the second ones and examined further. Moreover, common themes are discussed in view of a new understanding of the overall topic. If there are more memory-work groups involved, the findings and discussions are exchanged across the groups. This process of collective theorizing is a powerful feature of memory-work. These

findings are also related to other theoretical positions, which often leads to a reappraisal of their earlier understanding.

Memory-work has so far been mainly developed in the investigation of feminist topics like female sexualisation (Haug, 1987) or emotions and gender (Crawford et al., 1990). It has also been used in a more directly applied form by Susan Kippax (one the co-authors of the Crawford book) in the investigation of the memories men and women (hetero and homosexual) have of sexual contact as part of a project in HIV/AIDS research Kippax et al. In my own work I have successfully used this powerful research method with students in a course on intercultural learning (Schratz, 1994) and in a research methodology course with Rob Walker in an international peace-studies programme (cf Schratz & Walker, 1995).

In a single chapter it is not possible to reproduce the exciting and stimulating processes that go on in the life of a memory-work group. Nevertheless I wish to convey some of the liveliness we experienced in this work by presenting examples of students' memory stories, which had their origin in the peace studies program mentioned above. As a first step, the overall topic of education was broken into subtopics which formed possible themes for memory-work sessions. This was done by brain-storming in the whole group. After a thorough discussion, the whole group agreed on a theme that served as a starting point for the work in the small memory-work groups, each consisting of four to six co-researchers. Like Haug and Crawford, I found it was important to make this choice of theme collectively. Since memory-work is confronted with the problem of the uniqueness or singularity of any given experience, a collective decision ensures a commitment to generalizability. If people share a commitment to the theme, then each memory the group generates becomes a valid source of reference within the later discussions of the group. Therefore Haug et al. thought it possible to explore actual lived experience within social relations with a view to reaching generalizable conclusions.

> Since it is as individuals that we interpret and suffer our lives, our experiences appear unique and thus of no value for scientific analysis. The mass character of social processes is obliterated within the concept of individuality. Yet we believe that the notion of the uniqueness of experience and of the various ways in which it is consciously assessed is a fiction. The number of possibilities for action open to us is radically limited. We live according to a whole series of imperatives: social pressures, natural limitations, the imperative of economic survival, the given conditions of history and culture. Human beings produce their lives collectively. It is within the domain ot collective production that individual experience becomes possible. If therefore a given experience is possible, it is also subject to universalization. What we perceive as 'personal' ways of adapting to the social are also potentially generalizable modes of appropriation. Using our experience – in a positive as well as a negative sense – as an empirical base for our work thus offered the

possiblity of studying each individual mode of appropriation in detail. Our work derived its impetus from our recognition of the human potential to expand a capacity for action, to develop new possibilities. To enjoy diverse sensual pleasures; this was what led us to investigate the conditions of production of these pleasures, and to press for their universalization. Equally, we considered it vitally important to recognize recurrent forms of suffering in their specificity, in order to avoid reproducing them in the future (Haug, 1987: 43–44).

In view of these considerations we decided on themes which were open both to actual individual experiences and to generalizable conclusions for cross-cultural research. One of the themes chosen was, *'Being punished in school'*, a theme to which everybody could contribute and which was also relevant from an educational research perspective. Moreover, this theme seemed important because we all knew and understood the importance of repression in schooling. In the following, I present the first versions of memory stories on this topic.

He was 14 years old. It was his third year in high school. The rising bell went off at exactly 6 a.m., as usual. He got out of bed, changed into his working clothes and walked out of the dormitory towards the classroom area to perform his assigned morning duty.

It was a cold morning, so he walked back to the dormitory to get a cardigan. He opened his wardrobe and took out the cardigan. He was about to put on the cardigan when he heard the house captain ordering students out of the adjoining dormitory. He put on his cardigan and as he was just stepping out of his dormitory the house captain appeared at the entrance and immediately ordered him to go down on his knees. He refused, the captain who had a whip, threatened to use his whip if he didn't oblige. He stuck to his guns. The captain raised the whip to flog him. He grabbed the captain's hand and tried to force the whip out of his grip. At this moment, other prefects, captains intervened and took away the house captain. His classmates, who had been watching began to gigle and murmur amongst themselves.

Two hours later, one of the other prefects called him aside, and told him to go apologise to the house captain. he didn't. The next day the school principal sent for him and ordered him to trim the grass in the garden and also apologise to the house captain.

He was about 11 or 12. One of his subjects at primary school was Russian language. He was kind of good at it. One day the class got the task from the teacher to work in pairs and perform a Russian folk-tale in Russian, of course. The story was about a young boy who was kind enough to help an old man and in return he got a kind of a magic power: by saying some magic words he could fill any empty table with delicious food. That was the story.

So, he performed this story with his friend. The show went on quite smoothly, he enjoyed it. When they finally got to the part when he said the magic words, he – in order to imitate the appearing food – took out his sandwich from his pocket, which his mother prepared for him that morning/bread and butter with cheese and bologni/unpacked it and took a good bit of it.

The whole class was laughing, and he liked it; in fact that was his intention: To make a joke at the end of the story. But the teacher, a young man in his 30's, whose nickname was 'Camel' because of his funny walking, wasnt laughing. On the contrary, his face remained

very serious, he waited until the laughter was over and then he said: "So, you use the Russian class to consume your sandwich. I have to give you a 'one' [the worst grade in that school system] for undisciplined behaviour."

First he didn't really believe it, he thought the teacher was joking but then he saw him writing the grade in the class's report book. He sat down, he was shocked and he thought that the rest of the class was shocked too. He didn't cry continuously but some tears fell from his eyes and he had to blow his nose.

She was six years old; she had a one-year-older brother who was going to school, and she had to stay at home alone or with one of the parents.

The story happened in the beginning of the brother's education, somewhere in the first or second month of his attending the school.

The brother had the nice books for the first class, and she was very interested in what was covered in those books. She used to stay at home and trace the first letters from her brother's notebooks into her notebooks.

One day the brother got sick, he couldn't attend school, and she went to school instead of him. It was autumn and that day was heavily raining. The school wasn't so near to her home and she had to walk to school for more than half an hour. In the rain you had to go a longer way to school. She expressed her wish to her mother, who at first didn't want to let her go, but then she said: "You may go, but it is so far away, it's raining, it's a dangerous way beside the road, and you must be very careful. Your father could be very worried when he comes home, and maybe the teacher will not take you in – so maybe it is better for you to stay at home."

Her wish was stronger than the warnings, so she went. Her wish was even stronger than her shyness. The teacher didn't make any problems for her, and she was going to school for more than 15 days. The teacher and parents decided that it will be good for her to continue after her brother's recovery. The father went to school authorities in order to enroll his daughter in school.

One day she went with her father to school to receive the final decision of the school authority. The father went in one room and she had to wait outside. She hoped that everything would be fine. After a while her father went out with the school director. The father didn't say anything and she felt that something was wrong. All together they were going downstairs and the director all the time was saying stupid compliments to her. She didn't believe in him at all.

In the front of the school they were speaking a little and then the director came to her and said: "You are such a nice and beautiful little girl, I know that you like to play with children very much. You have to run, to play, to . . . Then you will come again here in one year, and then you can learn more." She was feeling resistance to each word, to each expression of that man's face. She couldn't understand what was the reason; even after her father told her the arguments which the director said to him.

She couldn't accept those arguments she felt that it is not right. She was sad when she realised that her father will give up. She was feeling helpless, she started to cry.

He was seven years old and in grade two of primary school. After he had had a very charismatic teacher in grade one whom he loved a lot, he even admired him, he was in this class with the new teacher. He had heard a lot about how strict that teacher was and that's why he'd been very afraid of moving on to second grade.

Right at the beginning of the school year Mr. Speckle, the teacher, had made clear that everybody had to obey his instructions. If one failed to do so one had to stand in the corner or, if it was something more serious, one had to reach out one's arms and he woudl use his rod on the hands. Soon after the beginning of the school year he started using the rod to show that he meant what he was talking about.

The boy was very afraid of being punished, so he tried not to behave in a way that he would have to either stand in the corner or be beaten on his hands. One day he was talking to his friend next to him because he had not understood the task they had to do. The teacher overheard the conversation but did not know who had been talking. Since nobody reacted when he asked who it was he sent four pupiles out in the right-hand corner in front of the room. The boy did not feel happy about the fact that others who had not been involved were punished, but he did not feel guilty himself since was was only asking a question. In the course of the lesson more pupils were sent to that corner so that it got quite crowded there eventually. The more people there were the more fun they got out of it and, looking at each other, they could hardly hide their fun about this.

It was only after some time that the teacher realised that this did not really do them any harm and he decided to ask them to the front of the classroom. They were all lined up and had to stretch out their arms, hands upside down. It was not until he got closer to him that the boy really became afraid of being beaten. When it was his turn and the teacher was reaching out with his rod he suddenly withdrew his arms, which the teacher noticed early enough so as not to hint into the air. Mr Speckle got very angry and shouted at him. He said he would never have expected him to do that. The boy had to reach out his arms again and the teacher hit him hard on each hand. Although it hurt a lot he was more ashamed about what the teacher had said.

It is not possible, here, to deal with all the steps and discussions within the framework of memory-work. After the first texts had been written the group started discussing and rewriting them according to the procedures mentioned in the description of the phases to follow (see above). On a more general level aspects of resistance and personal identity were worked out by the members of the group, which came out strongly in most of the texts. Moreover the areas of physical and psychological (structural) violence came to the foreground and formed a vital part in highlighting their marked effects on the identity formation process of young people through school. (For a more detailed account of the workings of memory-work see Schratz & Walker, 1995.)

NOTE

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